THURSDAY, AUGUST 29, 1878

THE ECLIPSE

THE telegram sent from Denver, signed by Professors Young and Watson, Dr. Draper, and myself, will have given an idea of the results which we think have been secured on the eclipse of this year. Since the telegram was despatched I have been engaged in making as many notes on the various points of detail as incessant travel and a temperature of 91° in the shade would permit. A long time must elapse before anything like a general view of the total work done is possible, but I think that the readers of NATURE may rely upon the correctness of what I have collected, though it is quite possible, as I have not succeeded in finding all the observers, that some errors may have crept in. And again, it is quite possible that the many dry plates taken and even yet not developed, may contain information of which at present we have no idea. To take an instance: we do not yet know whether Prof. Harkness's attempt to photograph the polariscopic phenomena presented by the corona was successful or not, for I learn at the Naval Observatory here that he is still at Fort Steele, handing over his camp equipment to the military, who all along the line have been placed in the most unreserved way at the disposal of the different parties by the express orders of Gen. Sherman himself, who takes the greatest interest in such inquiries, and is anxious to foster scientific inquiries as far as in him lies. Strange as it may seem, this is the expressed feeling of all the authorities here, from the Chief of the State downwards. In interviews with which I have been honoured, the President of the United States himself, the Secretary for War, Gen. Sherman, and other members of the Cabinet, have one and all insisted upon the importance of securing records of all possible natural phenomena, and expressed their gratification that such records have been secured in the present instance by Government aid.

I will begin the extracts from my notes by referring to the appearance of the Corona itself.

I give a rough sketch of what *I* saw of the corona with the naked eye (Fig. 1), slightly exaggerating the dimensions of the streamers to show the wind-vane appearance, which, to me, was almost perfect, being pointed at one end and bounded by parallel edges at the other; others, I may say, however, saw a resemblance to a fish's tail. These streamers vanished absolutely in the telescope (Fig. 2), as did the radiating lines in 1871; not a shred of them was left. Prof. Cleveland Abbe (lying on his back at a height of eleven feet on Pike's Peak; he had been sent down the day before from the summit, as the rarefaction of the air was too much for him) saw them, with the naked eye, extending to a distance of six degrees on either side, and their appearance suggested to him meteoric streams for their origin. Prof. Newcomb also saw them with the naked eye when he had hidden the corona behind a screen. To him they suggested the zodiacal light, or rather its nucleus near the sun, even if it extend beyond the orbit of the earth.

These streamers seem to have been seen by everybody, and were doubtless cosmical; a system at right angles to them (they lay along the ecliptic), and quite as bright, was recorded by many, though many of the best observers saw not a trace of them.

Here is a lesson, and one which affords an explanation of a great deal of eclipse work, connected with these outliers of the corona.

I had a magnificent view of the corona with a power of 50 on my 3¾-inch Cooke, and saw exquisite structure at the north and south points. Curves of contrary flexure started thence, and turned over, and blended with the rest of the corona, which was entirely structureless and cloud-like; the filamentous tracery which in India I observed till three minutes after totality had ended had all gone. Prof. Bass, however, tells me that by confining his attention to the same point for nearly the whole of totality, the structure came out, and seemed to pulsate like an aurora.

Prof. Hall is almost the only one who is under the impression that the corona of 1869 was less brilliant than this.

Mr. Burnham, who is an observer of the highest order, thus gives his opinion, which agrees with that of Prof. Young, who remarked its unusual faintness and lack of polar extension, and all the other American astronomers:—

"The coronal display was far less than in the eclipse of 1869, as seen at Des Moines, Iowa, by members of the *Times* party, and it bore a more striking resemblance, probably, to the eclipse of 1870, as seen at Gibraltar and in the Island of Sicily. The corona was, in fact, a mild affair, according to the observations of this party, as compared with that seen in other eclipses. A few protuberances were seen, and several bright streamers. On one side there was observed a bright pink ribbon or crescent. The traditional bright lines (the rays) and dark patches (the rifts) were not nearly as conspicuous as usual."

Mr. Burnham made another interesting observation which may prove one of considerable value in determining the nature of the corona. Anticipating that the star ζ Cancri would be very near to the sun, he made special efforts to see it, and was altogether successful, for it was distinctly seen through the corona.

General Myer, the distinguished head of the Army Signal Service, who had given orders to utilise his station on Pike's Peak for eclipse observations, observed the corona himself from the summit, and therefore, under conditions which have never been utilised before. He describes the corona as built up of five radial lines of a golden colour; beyond this in the direction of the ecliptic were prolonged bright silver rays. This was seen with the naked eye. In the telescope the appearance was quite different; a layer close to the sun, only of a light pink colour, was seen, and the long bright silver rays had disappeared. The greater elevation, thus, was more suited to a study of the structure than the lower levels, and at the same time the colour observed seems to have been slightly changed. In the pure air of the Peak, also, he saw the corona steadily for about five minutes after totality, and watched the moon cover the outer striated edge of the corona, which appeared, then, to be more golden than ever.

We next come to the Photographs.

Photographs of the corona and of its spectrum were

obtained at nearly all the stations. In order to give an idea of the extensive preparations which have been made, I may state what were Dr. Draper's photographic appliances as an instance of the equipment at one station.

In the first place there was a telescope of 5 inches aperture, and 78 inches focal length, especially corrected for photography, to get as complete a photograph of the corona as could be obtained by an exposure lasting

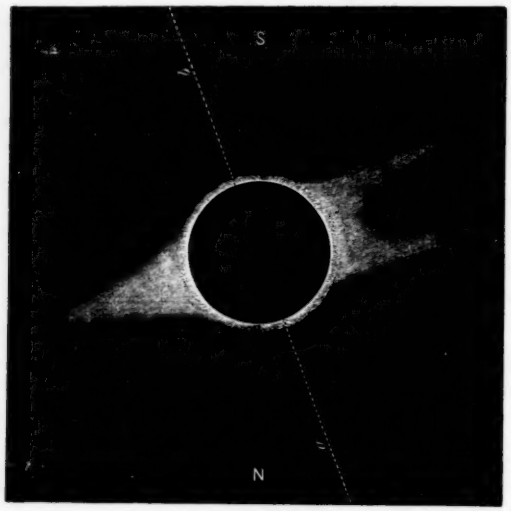

FIG. 1.—Naked eye.

during the whole of totality. This was during 165 seconds.

Next there was a large instrument which may be called a slitless "phototelespectroscope." This instrument consisted of an object-glass, composed of four lenses 6 inches in diameter and 21 inches focal length. The image of the sun at the focus was less than ¼ of an inch in diameter, and of extreme brilliancy. Before the rays of this lens reached the focus, however, they were intercepted by a Rutherfurd grating, about 2 inches square,

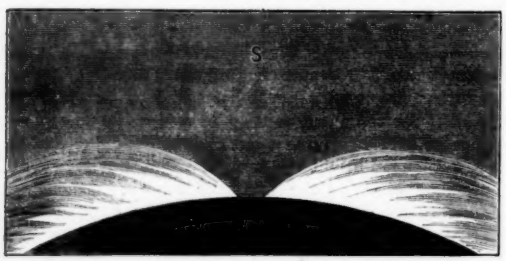

FIG. 2.—Telescope.

set at an angle of 60 degrees. This threw the rays to one side, and produced three images—a central one of the sun, and on either side of it a spectrum. One of these spectra was dispersed twice as much as the other, that is, would give a photograph of twice the length. This last photograph was actually about 2 inches long. With this instrument, mounted equatorially and driven by clockwork, if the light of the corona was due to gas, giving lines which lay in the actinic region of the spectrum, ring-formed images, one ring for each bright line, would be introduced. On the other hand, if the light of the corona arose from solid or liquid bodies, or was reflected light from the sun, a long band answering to the actinic region of the spectrum alone would be produced. If the light was partly from gas and partly reflected sunlight, a result partly of rings and partly a band would have been obtained.

As there was an impression that it was impossible to obtain a photograph of the spectrum of the corona, Dr. Draper, in order to give the best possible chance of

FIG. 3.—1, image of sun; 2, first order spectrum; 3, second order.

getting such a photograph, resorted to exceedingly sensitive materials, known as the lightning collodion process, furnished by the Messrs. Anthony, of New York. This involved a necessity of distilling a large part of the water used, because at Rawlins the water contains either alkali or sulphur, both of which are deleterious.

Since so many attempts were made, I had better confine my attention to the photographs. I have seen those of Prof. Hall and Dr. Draper. I will first deal with those of the corona itself. The former, taken by a dry-plate process, and, I believe, by means of a large apertured camera of short focus, are very admirable, and

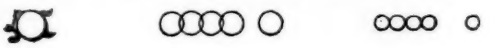

FIG. 4.

show the structure at the north and south points most exquisitely.

One of Dr. Draper's was exposed during the whole of totality, and represents a corona somewhat like those seen in the photographs of the 1871 eclipse taken with a medium exposure. In another, by a fortunate shake of the instrument, a trace of the outline of the chromosphere was received.

Next I come to the Spectra; but first I may be permitted to refer to the only photographic contrivance which was possible for my light marching order. This was a small portrait camera with a lens of one inch aperture,

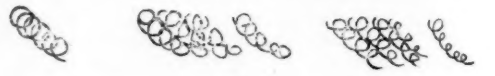

FIG. 5.

and a Rutherfurd grating of about 6,000 lines to the inch in front of it to act as a reflector and disperser.

I placed the camera and the attached grating on the ground in such a position that on the focusing screen I got the direct image of the sun at one edge of the plate, and running across the plate the spectra of the first and second orders on one side. A rough notion of what is seen on the plate when the sun is photographed thus will be gathered from Fig. 3.

Now if the corona had been built up of gases competent to give us lines with a spectroscope of ordinary construction, what I ought to get during an eclipse from

this instrument would have been something like Fig. 4, *provided I had a clockwork arrangement to drive it.* Without this contrivance the image and spectra will travel along the plate in the direction of the sun's motion—Let us suppose as here in Fig. 5.

It will be thus clear that, with this simple contrivance, the result obtained, if clock-work be employed, will be very similar in kind to that arrived at in Dr. Draper's more elaborate arrangement, to which attention has been drawn.

In the spectra, then, thus obtained, we had only one question to solve, as we all thought: we had to determine the position of the rings photographed in the first and second order spectra, or, at all events, in the first. Long before I developed my plate I knew what had happened. Dr. Draper, Prof. Thorpe, and myself, had photographed the spectrum—this I never doubted—but we had photographed as continuous a one as if a platinum wire, or ball, in a state of incandescence, had been in front of our instrument, instead of a shell of hydrogen gas 6′ high, as in 1869, and 10′ high, as in 1871.

Dr. Draper's plate was exposed during the whole of totality, to make assurance doubly sure. The continuous spectrum has no trace of a ring, though the plate was considerably over-exposed. In my own little photograph the continuous spectrum only is shown, and very clearly. While my plate was being exposed I held a duplicate grating close to the eye, expecting to see the rings as I saw them through five prisms in 1871. In first, second, and third order it was all the same—

Continuous spectrum,
Continuous spectrum,
Continuous spectrum;

and it was this which told me of the gigantic change which had taken place in the atmosphere of the great body we were studying.

The remarkable phenomenon thus photographed was of course as remarked by the eye observers; still bright lines were seen in the corona spectrum by one observer, using the term corona to represent everything outside the photosphere, and I make this remark because, whether the spectrum observed was that of the phenomenon which was visible or not I cannot say.

Prof. Young himself and many other observers saw the reversal of many lines at the moment of totality, thereby endorsing Pye and Maclear's observations of 1870 and 1871. After this, to all but Prof. Young the lines vanished utterly, and this was so absolute that in three parties there was consternation and fear, when nothing but a continuous spectra was seen, that something had gone wrong with the instruments.

Prof. Young saw 1474 very faint, *F without C*, and strangest of all, the H and K lines without the long calcium line. This is one of the most remarkable results of the eclipse, and must set students of solar physics to work on a new line. Young thinks the H and K lines were brilliant enough to suggest that the ring in this part of the spectrum photographed in the eclipse of 1875 was really due to them, and I am inclined to agree with him, though why the H and K lines should appear minus the longest line of calcium, passes comprehension, unless calcium is dissociated there from some cause of which at present we have no idea.

The discovery of a line in the ultra red by means of a thermopile, is another victory gained, and I sincerely hope that at the next eclipse Capt. Abney may use some "red molecules" to photograph it. Mr. Edison, with his wonderful tasimeter, also found indications of heat in the corona, but as he did not use a spectrum, he did not imitate Prof. Young in determining the exact position of the radiation.

We next come to the Dark Lines observed in the corona spectrum.

Janssen was the first to announce the fact that in the corona spectrum the chief Fraunhofer lines were to be seen. This observation was made on the Indian eclipse of 1871. The natural interpretation of this observation is that in the coronal atmosphere there are molecules sufficiently complex to reflect rays of every refrangibility, and that they do in reality reflect the solar light to us as it reaches them.

The observations on this point are a little doubtful, though it is held generally that the balance of evidence is in favour of their appearance. Prof. Barker, who observed with Dr. Draper at Rawlins, saw these lines at the moment that he was dumbfounded, as were most of the observers, at the absence of the bright ones. On the other hand at Prof. Newcomb's camp at Separation they were not traced, Commander Sampson seeing nothing but a perfectly continuous spectrum; 1474, F, and even C being utterly invisible. If the corona gives us light containing the Fraunhofer lines, it can only do so because it reflects solar light. Let us next see, then, what the evidence as to Polarisation is.

When all the observations on this subject are collected we may hope for much new knowledge, but the matter was not finally settled this time, for here again was a surprise.

Professors Morton and Hastings had arranged to determine the quality of the polarisation observed, Prof. Harkness was to attempt to photograph the phenomena, while Prof. Wright set himself the difficult task of quantifying it. One observer was specially told off to settle the questions raised by Prof. Pickering's observations of 1870. I may commence by saying that these last observations suggest that in 1870 the instrument used by Prof. Pickering was out of order, so that now everybody agrees that there is polarisation.

I have not yet been able to learn anything of the results obtained by Professors Wright and Harkness as the latter used dry plates which were to be developed here, and the Las Animas party took a long mountain trip to shake off the bad effects of a long camping on an alcali plain, and so did not put in an appearance at Denver.

Prof. Morton got radial polarisation most distinctly as I did in India in 1871. The *amari aliquid* is to be found in Prof. Hastings' results, which will be best seen from the following account obligingly placed at my disposal by Prof. Bass, one of his party:—

"The most important and unexpected result of the expedition was reached by Dr. Hastings in his polariscopic observations. It will be remembered that in previous expeditions, and by many observers in the present eclipse, only a few seconds were devoted to the examina-

tion of the corona for polarisation, and this by a bi-quartz or a Savart polariscope, held in the hand so as to take in the whole corona in one view. In the organisation of the expedition, Dr. Hastings was requested to prepare a plan by which the question as to whether the polarisation of the corona was radial or tangential, or absent, could be definitively settled. This was accomplished by an arrangement of apparatus essentially novel.

"The four-inch telescope had a Savart polariscope at its eye end. A diaphragm perforated with holes, 3′, 4′, 5′, 8′, 10′, 16′ was interposed between the polariscope and the objective, and the polarisation phenomena of a definite and limited portion of the corona were thus seen. Diaphragms of 3′ and 5′ were alone used. The telescope was pointed by Lieut. Very to six points, the first one above the sun and 50′ from the circumference; the second one 25′ from the circumference and 45° from the vertical; the third 1½′ from the sun and to the right of it; the fourth 16′ from the sun and to the right of it; the fifth 16′ from the sun and 135° from the vertical; the sixth 8′ from the sun and 135° from the vertical. These points were selected before the eclipse, but it was not known to Dr. Hastings in what order they would be viewed, so that the readings were absolutely free from bias. The position of the pointer of the polariscope was carefully verified before and after the totality. The readings were reduced immediately, and each reading (which was, as is evident, independent of every other) agreed in showing that the plane of polarisation was perpendicular to the sun's radius through the point examined—that is, *tangential*—a most unexpected result, about which, however, there is no doubt whatever. Four of the readings were made with a dark band in the centre, and two with a bright band. This result does not agree with others from the same eclipse, for the reason that in the method adopted by Prof. Morton, Mr. Lockyer, and others, there was, first, no means adopted for isolating a definite portion of the corona and determining its special polarisation. Second, that in the use of the Savart form, the phenomena of radial or tangential polarisation alike present bands radial to the sun. Third, that, with the bi-quartz, the field is so small that it becomes extremely difficult to interpret the tints of colour seen, if indeed any are seen. The novel fact was shown, by the use of this method, that the polarisation of the corona was exceedingly strong near to the sun's limb (one and a half minutes), and was relatively weak far from it."

I believe that no one is more astonished than Prof. Hastings himself at the result of his work, which, it has been laughingly said, goes to demonstrate the existence *ice crystals* in the corona. With reference to the remarks made on my 1871 work in the foregoing, I may add that I used a bi-quartz and a large field, so that the objections raised to the method do not apply.

This brings me to the end of my notes for the present on the photographic, spectroscopic, and polariscopic results obtained. Of course there was a whole world of wonders outside these fields of inquiry.

The view of the shadow of the moon from the summit of Pike's Peak cleaving its way along the lower air has been described to me as one of the most striking phenomena which it is possible to witness or imagine. The shadow, the boundary of which was seen curved, was preceded and followed by a spectrum. Where I was, nearly 8,000 feet high, and therefore a little over mid-height, there was no effect on the air, but after the close of totality the shadow was observed passing over clouds near the horizon. I may add, however, that the phenomena at the beginning and end of totality hardly existed for me, for I had to uncover and cover my photographic plates at those times.

It was not to be expected that in this country, where the anxiety for news and views seems always to be at fever heat, the astronomers would be allowed to quit their stations without giving an idea of the tendency of their work, and even its connection with the torrid temperature through which we have had to make our dusty way, or in which we have had to exist when locomotion had become impossible.

The utter disappearance of the large bright red corona of former years in favour of a smaller and white one in this year of minimum, struck everybody. Indeed it is a remarkable thing that after all our past study of eclipses this last one should have exhibited phenomena the least anticipated. It isolates the matter that gives us the continuous spectrum from the other known gaseous constituents. The present eclipse has accomplished, if nothing else, the excellent result of intensifying our knowledge concerning the running down of the solar energy. With the reduction of the number of spots or prominences for the last four years the terrestrial magnetism has been less energetic than it has been for the preceding forty years, while at both ends of this period we have had famines in India and China.

As the sun is the great prime mover of earth, and as every cloud, every air current depends upon it, its present quiet condition is worthy of the most minute study. The absence of lines from the corona spectrum shows a great reduction in the temperature of the sun, and such a marked change in the sun should produce a corresponding change on the earth, so that the associated terrestrial phenomena should be carefully observed. Hence I regard this eclipse as the most important that has been observed for many years as it throws much needed light on many points hitherto obscured in doubt.

Prof. Morton, of the Stevens Institute of Technology, remarked that the thing which impressed him most was the very curious character of the result; while on former occasions there has been projected into the space surrounding the sun a quantity of self-luminous gaseous matter which has no fixed place there, we now see that this was merely a temporary occupant which has, either by diffusion into space or absorption into the body of the sun, been removed. He also holds that the marked changes in the sun's condition would seem to call for corresponding marked changes in the condition of the earth. The results, in fact, recall to his mind one of the early theories of the maintenance of the sun's heat, which was that it was maintained by the impact of meteoric matter constantly falling in upon the sun from surrounding space. The quantity of heat produced by such an infall may be realised from the fact that a body so falling would develop by impact as much heat as 5,000 times its weight of carbon burnt in oxygen. Portions of the meteoric matter in the shape of meteorites fall upon the earth, and we thus know that most of them consist largely of iron, containing considerable quantities of absorbed gases; others are of a metallic character and devoid of such gases. If, now, we suppose that for a considerable period the sun's fires were fed with hydrogen-charged faggots, and then again that the main supply was of mineral matter, we might have at first a far-reaching atmosphere

of gas, such as has appeared hitherto, and at another time this might be absent, which seems to be the case at present.

He further adds that if such changes go on indefinitely it may not be irrational to inquire whether they may not in future produce such extraordinary climatic conditions in the earth as geology teaches us have existed in the ages of the past.

Prof. Young was careful not to commit himself to any decided connection between solar and terrestrial climatic changes; he, however, certainly concurs with me that the corona is fainter and the gaseous elements far less conspicuous than that observed at previous eclipses, and acknowledges that the different condition of the corona proves a change in the condition of the sun, as the corona acts with it in a sort of sympathy. Dr. Draper is resolute on the other side. He is reported to have said:—

"It is rather singular while the sun has been in such a quiescent condition for more than two years, that we have not seen more changes in the climate of the earth. This would seem to show that the abnormal condition of the sun at the maximum period of sun-spots, which occurs every eleven years, counts for but little against the total amount of heat that is sent out from the sun at all times. The present observations go to show that the activity or quiescence of the sun makes no perceptible difference in the earth's condition. I do not regard this most marked change in the corona as portending any change in the condition of either climate or crops."

Finally, on this whole question, I may remark that I have been not a little astonished to find how slowly European work percolates among the men of science here. I have met with few who are familiar with Meldrum's admirable work, and the discussion to which it has given rise. Still it is a great thing that at all events the cycle of solar changes has forced itself so markedly upon public attention.

For myself, as I have ever regarded sun-spots as

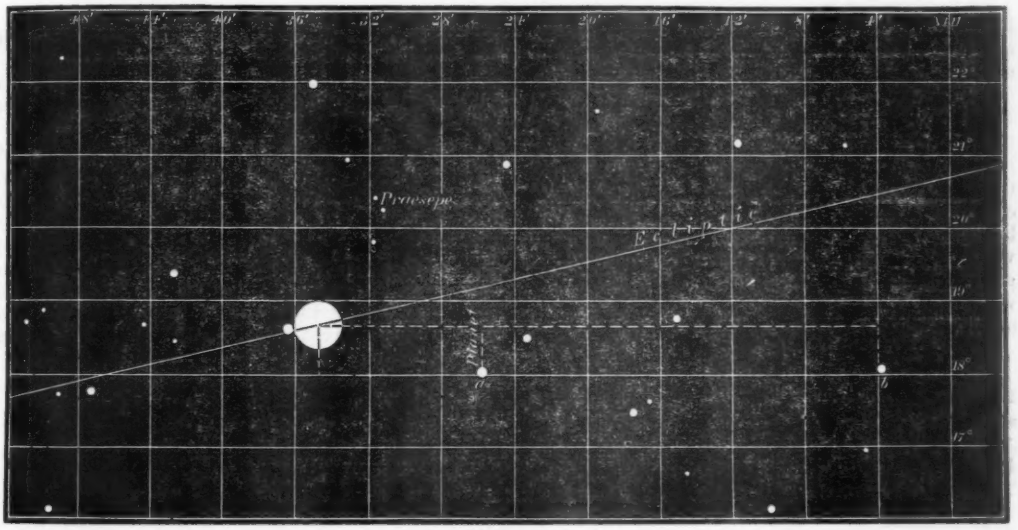

FIG. 2.

down-rushes—a term to which I still adhere—I am well content to see this view indorsed by such a chain of facts as the corona has now supplied. In spot-maximum years we have violent up-rushes of gas from the sun's interior, and the corona is mainly built up of such gas. Further, we have spots, and, if these are not evidences of the return convection currents, we have none other. In spot-minimum years, such as the present, we have no up-rushes, and the corona contains no gas, and there are no spots. Spots, then, are only observed when we have a right to look for the return of the upward current, about which there is no doubt, and the rate of which we have measured.

But if this puts beyond all question, as I hold it does, the nature of spots, on the other hand, the separation of the gaseous from the continuous spectrum of the corona indicates that we have yet much to learn of the temperature and nature of the corona when the spots are absent.

So much, then, touching the progress of solar matters during the eclipse of 1878. I have not yet, however, done with the observations.

There is little doubt, I think, that an intra-Mercuria planet has been found by Prof. Watson. If it will fit one of Leverrier's orbits, and should turn out to be Vulcan, no doubt astronomers will be able to keep a firm grasp upon it, and sooner or later its elements will be determined.

Prof. Watson, of Ann Arbor, whose belt, as the papers here put it, is graced with the scalps of I know not how many minor planets, broke off work on a planet beyond Neptune to come to discover one inside Mercury. He went with me in Mr. Silvis' railway photographic car from Rawlins to Separation on the morning of the eclipse, intending to observe with me at the station we were determined to occupy, with our light equipments, as the number of detached clouds visible at the time of totality on the pre-

vious days had strongly shown the advantage of separating the parties as much as possible. We chose a spot to leeward of one of the enormous water tanks of the Union Pacific Railway, which form the chief features in the interesting but desolate plains in that region, over which the wind sweeps at times with incredible violence.

On reaching our destination we found Prof. Newcomb, whose camp was about a mile away, and it was then agreed that as both he and Prof. Watson were to hunt for the planet they had better be together, so I lost his company during the eclipse.

Prof. Watson's plan of operation was to sweep south of the sun and observe all the stars in the map, a part of which is here reproduced (Fig. 6), and to refer the position of any new body to the stars, or, if possible, to the sun itself. For this purpose, with the assistance of the Rawlins carpenter, he armed his equatorial with paper circles and brass wire pointers. He commenced operations to the left of the sun and saw the stars marked, but none others. Then sweeping out to the star marked *b* he noticed on his return another not on the chart, marked *a*. He then made three marks on his right ascension paper circle, on the spots occupied by the pointer, when the sun, *a* and *b*, were successively brought into the centre of the field. He next determined the difference of declination in the same way between the sun and *a*, having the additional help that *a* was nearly in the same declination as *b*. He then repeated his R.A. measures, and called Prof. Newcomb, but the eclipse was over before anything more could be done. I give this statement from memory only (as I was too busy to make notes at the time), as I heard it soon after the eclipse at the camp, before the telescope was dismounted. It is probable that subsequent careful measures of the circles may alter the place—

R.A. 8h. 26m.
Dec. + 18° 00′

I telegraphed to you, somewhat, but the alteration will be small.[1]

Since arriving here I have learned that Mr. Swift, of Rochester, a well tried observer, also saw the planet. The first account I read of his work was as follows:—

"This gentleman made a very careful search for Vulcan, scanning the heavens very closely with his splendid comet eye-piece, made by the celebrated Gundlach, but he saw nothing of it. He did, however, see, about three degrees from the sun, two stars not down in the charts or star maps, and about as bright as the pole star—they were pointing directly towards the sun. On attempting to re-find them, he was prevented by a little cloud."

Since then, however, another fuller account of his work has appeared, from which I gather that about one minute after the commencement of totality two stars caught his (Mr. Swift's) eye about three degrees, by estimation, south-west of the sun. He saw them twice, and attempted a third observation, but a small cloud obscured the locality. The stars were both of the fifth magnitude, and but one is on the chart of the heavens. This star he recognised as Theta in Cancer. The two stars were about eight minutes apart. There is no such configuration of stars in the constellation of Cancer. In 1859 the French astronomer, Lescarbault, claimed that he had seen an intra-Mercurial planet crossing the sun's disc. He related his discovery to Leverrier, who became a firm believer in the existence of such a planet. The perturbations of Mercury's orbit demand such a planet as Leverrier named Vulcan. The star Mr. Swift saw may have been the same that was seen by Prof. Watson, who was located at Rawlins, Wy. T.

Mr. Swift possessed a comet eye-piece of very flat and large field, and distinct to the very edge. It was made in Rochester, and to it and his blunder in failing to untie his instrument, he believes he owes his success.

The instrument used in the search for Vulcan by Prof. Holden proved to be inadequate to show all the stars on the Washington star map, owing to the brightness of the corona. The space where the planet seen by Prof. Watson was, was four times swept over, but so near to the sun as this, a four and a-half magnitude star was not to be seen. A space of 10° in declination by 35° in right ascension was twice swept over.

Here, again, we get an idea of the thoroughness with which the work has been planned and executed.

It would be wrong to conclude these hurried notes without stating that, from the day in which I landed in New York to the present time, I have become everybody's debtor for acts of kindness, which have touched me greatly. This great country is a land of true courtesy, for which I here express my gratitude, not only to my scientific brethren, and chiefly to Dr. Draper, General Myer, and Prof. Newcomb, whose guest I have been, but to hundreds to whom I have been a stranger and unknown.

As significant of the keen interest taken in the eclipse by all classes here, I may mention, in conclusion, that on the Sunday before the event prayers for fine weather were offered in all the churches of Denver.

Washington, D.C., Aug. 8 J. NORMAN LOCKYER

AS I have been recently giving attention to the subject of solar spectroscopy in consequence of my discovery of oxygen in the sun, it seemed desirable to take advantage of the total eclipse of July 29, to gain as precise an idea as possible of the nature of the corona, because the study of that envelope has been regarded as impossible at other times. The main point to ascertain was whether the corona was an incandescent gas shining by its own light, or whether it shone by reflected sunlight.

For this purpose I organised an expedition, and was fortunate enough to secure the co-operation of my friends Professors Barker and Morton, and Mr. Edison. The scheme of operation was as follows: (1) the photographic and photo-spectroscopic work, as well as the eye slitless spectroscope were to be in charge of my wife and myself; (2) the analysing slit spectroscope was in charge of Prof. Barker, with the especial object of ascertaining the presence of bright lines or else of dark Frauenhofer lines in the corona; (3) the polariscopic examinations were confided to Prof. Morton, who was also to spend a few moments in looking for bright or dark lines with a hand spectroscope; (4) Mr. Edison carried with him one of his newly-invented tasimeters with the batteries, resistance-coils, Thomson's galvanometer, &c., required to determine whether the heat of the corona could be measured.

This entire programme was successfully carried out,

[1] On going to press we receive a letter from Prof. Watson, dated Ann Arbor, August 14, stating that the result of more careful examination gives—Washington M.T., July 29, 5h. 16.m., R.A, 8h. 26m. 54s., Dec. + 18° 16′.

and good fortune attended us in every particular. The results obtained were: (1) the spectrum of the corona was photographed and shown to be of the same character as that of the sun, and not due to a special incandescent gas; (2) a fine photograph of the corona was obtained, extending in some parts to a height of more than twenty minutes of arc, that is, more than 500,000 miles; (3) the Frauenhofer dark lines were observed by both Professors Barker and Morton in the corona; (4) the polarisation was shown by Prof. Morton to be such as would answer to reflected solar light; (5) Mr. Edison found that the heat of the corona was sufficient to send the index beam of light entirely off the scale of the galvanometer. Some negative results were also reached, the principal one being that the 1474 K, or so-called corona, line was either very faint or else not present at all in the upper part of the corona, because it could not be observed with a slitless spectroscope, and the slit spectroscope only showed it close to the sun.

The general conclusion that follows from these results is that on this occasion we have ascertained the true nature of the corona, viz., it shines by light reflected from the sun by a cloud of meteors surrounding that luminary, and that on former occasions it has been infiltrated with materials thrown up from the chromosphere, notably with the 1474 matter and hydrogen. As the chromosphere is now quiescent this infiltration has taken place to a scarcely perceptible degree recently. This explanation of the nature of the corona reconciles itself so well with many facts that have been difficult to explain, such as the low pressure at the surface of the sun, that it gains thereby additional strength.

The station occupied by my temporary observatory was Rawlins (lat. 41° 48′ 50″, long. 2h. om. 44s. W. of Washington, height 6,732 feet above the sea), on the line of the Union Pacific Railroad; because, while it was near the central line of totality, it had also the advantages of being supplied with water from the granite of Cherokee mountain, and of having a repair shop, where mechanical work could be done. I knew by former experience that the air there was dry and apt to be cloudless; in this particular our anticipations were more than fulfilled by the event, for the day of totality was almost without a cloud and the dew-point was more than 34° F. below the temperature.

The instruments we took with us were as follows, and weighed altogether almost a ton:—1st. An equatorial mounting, with spring governor driving clock, lent by Prof. Pickering, Director of Harvard Observatory. 2nd. A telescope of 5¼ inches aperture and 78 inches focal length, furnished with a lens specially corrected for photography, by Alvan Clark and Sons. 3rd. A quadruple achromatic objective of 6 inches aperture and 21 inches focal length, lent by Messrs. E. and H. T. Anthony, of New York. To this lens was attached a Rutherfurd diffraction grating nearly 2 inches square, ruled on speculum metal. The arrangement with its plate-holders, &c., will be designated as a phototelespectroscope. 4th. A 4-inch achromatic telescope with Merz direct vision spectroscope, brought by Prof. Barker from the collection of the University of Pennsylvania. 5th. A 4-inch achromatic telescope, also brought by Prof. Barker; to it was attached Edison's tasimeter. Besides these there were polariscopes, a grating spectroscope, an eye slitless spectroscope with 2-inch telescope, and finally, a full set of chemicals for Anthony's lightning collodion process, which, in my experience, is fully three times quicker than any other process.

The arrangement of the phototelespectroscope requires further description, for success in the work it was intended to do, viz., photographing the diffraction spectrum of the corona, was difficult, and, in the opinion of many of my friends, impossible. In order to have every chance of success it is necessary to procure a lens of large aperture and the shortest attainable focal length, and to have a grating of the largest size adjusted in such a way as to utilise the beam of light to the best advantage. Moreover, the apparatus must be mounted equatorially and driven by clockwork, so that the exposure may last for the whole time of totality, and the photographic work must be done by the most sensitive wet process. After some experiments during the summer of 1877 and the spring of 1878 the following form was adopted:—

The lens being of 6 inches aperture and 21 inches focal length, gave an image of the sun less than ¼ of an inch in diameter and of extreme brilliancy. Before the beam of light from the lens reached a focus it was intercepted by the Rutherfurd grating set at an angle of 60 degrees. This threw the beam on one side and produced there three images—a central one of the sun, and on either side of it a spectrum; these were received on three separate sensitive plates. One of these spectra was dispersed twice as much as the other, that is, gave a photograph twice as long. This last photograph was actually about two inches long in the actinic region. If now the light of the corona was from incandescent gas giving bright lines which lay in the actinic region of the spectrum, I should have procured ring-formed images, one ring for each bright line. On the other hand if the light of the corona arose from incandescent solid or liquid bodies, or was reflected light from the sun, I was certain to obtain a long band in my photograph answering to the actinic region of the spectrum. If the light was partly from gas and partly from reflected sunlight, a result partly of rings and partly a band would have appeared.

Immediately after the totality was over and on developing the photographs, I found that the spectrum photographs were continuous bands without the least trace of a ring. I was not surprised at this result because during the totality I had the opportunity of studying the corona through a telescope arranged in substantially the same way as the phototelespectroscope, and saw no sign of a ring.

The plain photograph of the corona taken with my large equatorial (exposure 150 seconds) on this occasion shows that the corona is not arranged centrally with regard to the sun. The great mass of the matter lies in the plane of the ecliptic but not equally distributed. To the eye it extended about a degree and-a-half from the sun toward the west, while it was scarcely a degree in length toward the east. The mass of meteors, if such be the construction of the corona, is therefore probably arranged in an elliptical form round the sun.

For the fortunate results of this expedition we are not a little indebted to the railroad and express companies. The Pennsylvania, the Chicago and North-western, and the

Union Pacific railroads, the Pullman Palace-Car Company, and the American and Union Pacific Express companies made the most liberal arrangements, and Mr. Galbraith, the superintendent of the Repair Works at Rawlins, gave us the free use of his private house and grounds. Of the citizens of Rawlins it is only necessary to say that we never even put the lock on the door of the observatory, and not a thing was disturbed or misplaced during our ten days' residence, though we had many visitors. They sent us away with a serenade. HENRY DRAPER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Floating of Solid on Molten Metal

I OBSERVE in NATURE (vol. xviii. p. 397) a note of some experiments on this subject. The results of these experiments (unless with lead) are, I think, very similar to some which I have made, and described in your pages (see NATURE, vol. xvi. p. 23), viz., that with *heavy pieces* the metal first *sank* and then *rose to surface;* with light pieces the "skin" formed on the surface of the ladle was sufficient to keep them afloat. From these experiments I drew the conclusion that the cold solid metal was specifically *heavier* than the molten metal, but after a short immersion, depending on size of pieces, these pieces had *expanded* by the great heat around them so as to have *their bulk increased sufficiently* to enable them to float. My experiments with solid pieces of lead showed that they *sank* and did not come to the surface, and could be felt lying at bottom. Pieces of sheet lead rolled up floated.

In some recent experiments which I made, I found that *cold* pieces of steel rails placed in a furnace of molten steel sunk at first and floated afterwards, but that *hot pieces floated*, and did not sink. W. J. MILLAR

100, Wellington Street, Glasgow, August 10

A Meteorite?

THIS day, at 12.15 P.M., I was considerably startled by what was to me a remarkable phenomenon. The weather had been very "thundery" all the morning, and heavy rain was falling in torrents. I sat at my desk by a window looking out upon a court inclosed by high walls. Chancing to look out of the window I heard a sharp report, just like the crack of a Snider rifle sounding immediately outside, followed instantaneously by the descent of a ball of fire about the size of an ordinary gas-lamp globe. This fell vertically and with lightning speed, but when just on a line with the centre of my window burst into a splendid mass of rays, whitish-blue in colour, and of dazzling brilliancy. That is all I can tell you about it. Every one in the house heard the report, and quite a temporary panic ensued. No material effect of the meteorite's presence can be found.

Perhaps some of your readers may be able to explain the phenomenon. J. HARRIS STONE

67, Chandos Street, Strand, August 23

The Australian Monotremes

THE *Tachyglossus* was shot by me near Georgetown, in lat. 18° S. I have found it inhabiting the porphyritic ranges (Newcastle and Mount Turner) in this locality, where they are rather numerous. In my letter (NATURE, vol. xvi. p. 420) I should have written "integumentary" pouch. The *Orinthorhynchus paradoxus* I saw floating with its bill above water in a lagoon between Georgetown and Normanton, 150 miles west of the former town.

Owing to the noise made by my detachment in riding up, I was unable to capture this specimen, but I do not despair of securing one on my next trip westward. I certainly believe the *Tachyglossus* extends throughout the length of the Cape York peninsula on the east, and through the Gregory, Leichhardt, and Cloncurry ranges to the southward and south-westward of Georgetown. Its habit of burrowing beneath rocks precludes the possibility of its occurrence on the Lower Gilbert and Flinders River plains.

"P. L. S." will find my "notes" on this subject in the Linnæan Society's *Journal*, as I sent them, accompanied by the skull of an adult female, to the Society in March last.

Georgetown, June 1 W. E. A.

Microphone in Indirect Circuit

IT is not absolutely necessary that the microphone should form part of the direct circuit. It works just as well if connected so that, when the carbons are not touching, the whole of the current goes through the telephone. When the carbons are together a small portion will of course leak through them; upon this leakage depends the rise and fall of tension in the receiver. For some experiments it is even better to work the microphone in this indirect manner, as the circuit always remains closed, and prevents, in a great measure, the *jarring* noise resulting from a break. ALFRED CHIDDEY

Bristol Mining School, August 19

OUR ASTRONOMICAL COLUMN

THE SATELLITE OF NEPTUNE.—We here present in a tabular form the means of determining the approximate position and distance of the satellite of Neptune, with respect to the primary for any time during the months of September and October, or indeed by extending the epochs subjoined, for any time during the present opposition. The argument u is the distance of the satellite from the ascending node of the orbit upon the earth's equator, and $u = 0°$, at these Greenwich mean times:—

	h.	m.		h.	m.
Sept. 4 ...	18	38·3	Oct. 4 ...	3	52·0
,, 10 ...	15	41·0	,, 10 ...	0	54·7
,, 16 ...	12	43·8	,, 15 ...	21	57·5
,, 22 ...	9	46·5	,, 21 ...	19	0·2
,, 28 ...	6	49·3	,, 27 ...	16	3·0

The motion of u in one day is 61°·257, in one hour 2°·552, and in one minute 0°·0425. Having determined the value of u from these epochs and motions for the proposed time of observation, the angle of position and distance of the satellite from the centre of the planet may be taken from the following table, in which the first and second columns of angles apply to the respective columns of the argument u:—

Arg. u.		Angle of position.		Distance.	Arg. u.		Angle of position.		Distance.
°	°	°	°	″	°	°	°	°	″
0	180	71·6	251·6	9·8	90	270	29·8	209·8	15·0
10	190	63·1	243·1	11·7	100	280	25·0	205·0	13·6
20	200	56·9	236·9	13·5	110	290	18·9	198·9	11·8
30	210	52·0	232·0	14·9	120	300	10·6	190·6	9·9
40	220	48·0	228·0	16·0	130	310	358·2	178·2	8·0
50	230	44·3	224·3	16·7	140	320	338·9	158·9	6·4
60	240	40·9	220·9	17·0	150	330	311·7	131·7	5·8
70	250	37·5	217·5	16·8	160	340	284·2	104·2	6·4
80	260	33·8	213·8	16·1	170	350	264·4	84·4	7·9
90	270	29·8	209·8	15·0	180	360	251·6	71·6	9·8

The period of revolution of the satellite is 5d. 21h. 2·74m., and by successive additions of this period the epochs may be continued for November or later.

As an example of the application of the table, suppose it is desired to know the approximate position of the satellite on September 14 at Greenwich midnight. Strictly the time for aberration should be deducted, which, in minutes, is given by [0·9189] × log. distance of Neptune from the earth—this log. distance being taken from p. 269 of the *Nautical Almanac*. In the present case we find 4h. 1·9m. to be deducted from 12h., so that

the time to be used in the calculation is September 14, 7h. 58·1m. This time follows the epoch in the above table by 2d. 13h. 36·4m., and with the motions already given, the value of u corresponding to this interval is found to be 225°·34, with which we enter the table and find the angle of position 226°, and the distance 16″·4. A direct calculation from Prof. Newcomb's tables of the satellite for the reduced Greenwich time gives 226°·4 and 16″·4.

NOTES

THE Chemical Society have lately made the following grants from their Research Fund:—50*l.* to Dr. Tilden, for an investigation into the chemical nature of the terpenes; 50*l.* to Mr. W. N. Hartley, for apparatus and materials required in carrying on a research on the action of organic substances on the ultra-violet rays; 30*l.* to Dr. W. Ramsay, for determining the electric conductivity and resistance of solutions of salts at different temperatures; 20*l.* to Mr. W. Jago, for the purchase of apparatus required for carrying on a research on the organic matter in sea-water; 10*l.* to Mr. W. A. Shenstone, for an examination of certain reactions of brucine and strychnine.

IT is understood that Signor d'Albertis has parted with the whole of the extensive zoological collections made during his two last expeditions up the Fly River, New Guinea, to the Marquis G. Doria, of Genoa, who will, no doubt, deposit them in the Museo Civico of that city, of which he is the founder and principal benefactor. These collections were first offered to the British Museum. Signor d'Albertis is now making arrangements for the publication in London of a narrative of his adventures in New Guinea.

WE regret to hear that the valuable collection of Chinese birds made by the late Mr. Robert Swinhoe, F.R.S., is still undisposed of. It was offered, we are told, to the British Museum, but, as too often happens in such cases, declined. It would be greatly to be lamented if this collection, which contains many types of species first described by Mr. Swinhoe, and the originals of his numerous papers on Chinese ornithology, should pass out of the country. It certainly ought to have been acquired for the national collection, even if a little more than what was considered its full value had had to be paid for it.

WE understand that the authors of the "Unseen Universe" are at present engaged on a work intended to serve as a sequel to that well-known volume.

WE are requested to state that Sir Joseph Whitworth having expressed a desire that some important alterations should be made in the conditions of his scholarships, the detailed rules for carrying out his wishes are now under consideration. They will be published as soon as possible; but in order to prevent disappointment this notice is given. No important changes will be made in the conditions of the competitive examination in May, 1879. But the conditions of the tenure and of the amount of the scholarships may be somewhat modified.

THE meeting of the French Association for the Advancement of Science was inaugurated in the large hall of the Sorbonne by an address by M. Fremy on Soda and Steel in 1878. The address was well received by a large audience, but it is generally regretted that the president did not take a subject of wider bearing for his address. MM. Virchow and Haeckel were present during the address. It is the first time that German men of science have taken part in French public assemblies in their private capacity. M. Gambetta was also present. Commandant Perrier gave a sketch of the work done at the last year's meeting of the Association. The number of members is now 2,384, the income for 1877 was 58,000 francs, and its capital 224,897 francs. The grants for 1877 amounted to 13,850 francs. A part of the effect of this scientific assembly is lost this year on account of the number at the Trocadéro, and the multitude of objects demanding the attention of Paris visitors. The morning sittings of the Meteorological Section are held at the Lycée St. Louis, and the evening meeting at the Trocadéro as a Congress of the Exhibition. One of the most interesting addresses will no doubt be that delivered by Dr. Janssen on celestial physics; and he will also give the substance of the papers read by him before the British Association. There are two candidates for the presidentship of the meeting of 1880, M. Krantz, Director of the Exhibition, and Col. Laussedat, of the Engineer service. Very probably the former will be successful. Several cities are competing for the honour of receiving the Association in 1880. It seems probable that it will cross the Mediterranean to Algiers; M. Krantz is spoken of as the intended Governor-general of Algiers after the close of the Exhibition. Next year's meeting will take place at Montpellier under the presidency of M. Bardoux, the Minister of Public Instruction. On Sunday the Congress visited the Paris Observatory and the Meteorological Observatory at Montsouris.

AT the general meeting of the International Botanical and Horticultural Congress held at the Trocadéro, Paris, on the 16th inst., M. A. de Candolle was elected president. A *soirée* or reception was held on the evening of the 16th, and the members of the Congress went on the 18th to Sagrez, the residence of M. A. de Lavallée, president of the organising committee, by whom they were most hospitably entertained. The collection of hardy woody plants at Sagrez is unique in completeness in every sense.

ON Thursday last Mr. Cunliffe Owen was entertained at luncheon by some of the leading U.S. exhibitors at the Paris Exhibition, on board Capt. Boyton's yacht. Governor M'Cormack, in proposing Mr. Owen's health, spoke very highly of his executive abilities, and his courtesy towards the Americans, and attributes much of the success of the exhibits of English-speaking people to his energy, skill, tact, and good management.

IN the *Times* of August 13 an account is given of the working of the Mallory propeller, a very ingenious mechanical device for propelling and steering a ship in any direction by means of one and the same apparatus, recently brought over to England from America by its inventor, Col. William H. Mallory, of the United States army. It consists of a vertically swivelled screw propeller of peculiar construction, by the aid of which a vessel can be moved sideways, turned rapidly on a circle whose diameter is the ship's length, and can be made to advance or retire with equal rapidity. We cannot see any reason why such a propeller may not be very useful for small vessels and others whose turning is more important than speed and sea-going qualities. That it will ever replace the direct propeller seems improbable, as there are three distinctly bad points about it:—1, the gear, which entails a loss of at least ten per cent. of the power; 2, the engines working on to a vertical shaft; 3, the engines being over the counters, which would render the ship very uneasy in a rough sea. Besides this, no vessel would go to sea without a rudder to use in case her engines broke down. On the other hand, the propeller presents many advantages for harbour work, and for such things as rams and torpedo boats.

WE learn from *Harper's Weekly* that, for the purpose of prosecuting biological researches, Prof. A. Agassiz has lately completed a superb establishment near his residence at Newport, wherein every device that experience could suggest has been brought to bear for the convenience of investigators. A building 45 by 25 feet has been erected on the side of a bay making up from the entrance to Newport Harbour, and provided with the purest of sea-water by means of a steam-pump, which keeps a tank constantly filled. The tables are covered with a series of

tiles of different colours, so that the minute animals of different shades can be the more readily overhauled when emptied upon them. The shelves in the laboratory are all of glass, the tanks are of slate, the conducting pipes are of iron, lined with a composition of rubber, which it is believed will protect them against corrosion. These tables are all well lighted, and are available for students, whom Mr. Agassiz invites to share his facilities. Four persons, in addition to himself, at present occupy the laboratory in prosecuting their special researches. There is, probably, no one of the many buildings erected here and in Europe for the prosecution of biological research so elegantly and thoroughly equipped as that to which we refer.

PROF. F. E. NIPHER, of Washington (U.S.) University, has undertaken a magnetic survey of the state in connection with the weather service. The work will require about three years to complete, and he contemplates getting over one-third of the state this summer. He will establish about twenty or twenty-five magnetic stations, to determine the dip of the magnetic needle and the deflection from the magnetic meridian. The professor started out on the survey early in July, accompanied by five or six students from Washington University, who will act as the assistants. He has received a complete set of instruments from the United States Coast Survey, consisting of a dip-needle and declination needle, with theodolite. The party will first go to St. Charles, and up the North Missouri railroad, taking in a territory as far west as Chillicothe, and embracing among the points of observation Hannibal, Macon City, Mexico, Columbia, Fulton, Kirksville, Moberley, and other eligible points.

MR. AUGUSTUS FENDLER, whose collections of plants, made thirty years ago in the vicinity of Santa Fé, were made known by Prof. Gray, and who for many years resided in Venezuela as a botanical collector, is now prosecuting similar work in the Island of Trinidad, devoting himself to the gathering of ferns and fern-like plants. Sets of the first series of these, embracing the first thirty-eight species, are procurable at a moderate price from the curator of Harvard University Herbarium, Cambridge, Massachusetts. They have all been named by Prof. Eaton, of Yale College.

SOME months ago we gave an account of the explorations of a young American, Mr. Herbert H. Smith, on the Amazon, referring to some geographical discoveries made by him, as also to his success in securing a large series of insects. Mr. Smith left New York on July 6 for Brazil, first to complete his work on the Amazon, and then to proceed to the southern provinces of Brazil.

A VIOLENT earthquake is reported from Innspruck. It occurred on the 9th inst. at 12.40 P.M., and was accompanied by loud subterranean noise. At 9 A.M. on the 26th, earthquake shocks were felt over a great part of Belgium and Holland, and in Rhenish Prussia at about 11 A.M. A shock of earthquake was felt at nine on the morning of the 26th inst. at Liége, doors and windows being much shaken and chairs disturbed. It was also felt at Elberfeld, Cologne, Osnabrück, and Barmen. At Barmen houses were upreared, roofs displaced, furniture shaken, and goods in the shop windows overturned. The shocks were repeated about eleven at Elberfeld, Düsseldorf, Cologne, and Bonn.

AT Halberstadt a remarkable Fata Morgana was observed on the 5th inst. at 7.10 P.M. The phenomenon is described by an eye-witness, who states that in a stratum of cloud in the direction of the Brocken both house and tower standing on the summit of that mountain were reproduced in distinct outlines and on a gigantic scale; even the windows could be recognised. The duration of the phenomenon was about one minute.

THE meeting of German anthropologists took place at Kiel on the 12th, 13th, and 14th inst., and was well attended. Strassburg was chosen as the meeting-place for next year. Prof. Schaaffhausen of Bonn presided, and addresses were delivered by Professors Handelmann, Mehlis, Fraas, Virchow, Ranke, Stieda, and others. On August 14 an excursion was made to Lübeck. The Schleswig-Holstein Anthropological Society had arranged an interesting exhibition for the occasion.

THE British Archæological Association concluded a very successful meeting at Wisbeach on Saturday. Under the presidency of the Bishop of St. David's, the Cambrian Archæological Association held its annual gathering at Lampeter last week.

A CANTON correspondent, under date April 12, sends us the following account of the tornado of April 11 :—Yesterday afternoon, April 11, Canton was visited by a whirlwind of terrific force and unparalleled destructiveness. There was a thunderstorm from 2 P.M. to 3.30; when this was over, lumps of ice, about the size of pigeons' eggs, but shaped like the star-fish, fell in great quantities. Our surprise was hardly over at this strange phenomenon (thermometer 80° F. in the shade) when a noise was heard, like that made by the screw of a ship as a person on board sleeping close to it would hear it, rising and falling in regular rhythm. This sound was occasioned by the travelling of a wind of an intensity that baffles description, which burst upon the city and settlement, scathing and blasting everything which came within its fell grasp. For a space of time, variously estimated at from three to five minutes, it performed its work of destruction, uprooting trees, unroofing houses, overturning walls, engulfing boats, and leaving behind a scene of desolation such as only an eye-witness would believe in. The path along which the tornado passed was converted into shapeless ruins, but fortunately its width was not more than 200 yards. In Shamien (the foreign settlement) 134 large banyans, some of eighteen years' growth, have been blown down, most of them torn up by the roots, and in rare cases the trunks have been rudely snapped in two. A large banyan, distant twenty yards from our house, was torn up and hurled roots foremost, right into the verandah, smashing to chips the solid stone balustrade. One single instance will give you a fair idea of the force of this hurricane. A tile was found sticking in the side of a tree (in the Consulate grounds), into which it had penetrated two inches. A coolie was killed on the road fifty yards from our house by a brick blown away. Men were blown yards into the air and killed, and in one instance, a cow was blown up, but was not killed. The stone seats in the Bund, and stones over a ton weight have been driven yards away. The immense granite blocks forming the parapets of the bridge were hurled bodily into the canal. The track followed by the tornado through the native city is marked by a confused mass of bricks and mortar, and in some places there is literally "not left one stone standing on another." According to a native estimate the number of houses entirely destroyed is about 5,000, the total damage to *foreign* houses about 12,000*l.* sterling, and the loss of life 6,000. Already 4,000 corpses have been interred, and more are being dug up every minute.

WE have received the first part (*Polypetalæ*) of a "Diagnoses Plantarum Novarum vel minus Cognitarum Mexicanarum et Centrali-Americanarum," by Mr. W. B. Hemsley. This, we believe, is merely the forerunner of a work of great extent and importance which Mr. Hemsley has undertaken, of the scope of which the following statement may give some idea. It may be known to some of our readers that Messrs. Godman and Salvin, aided by various specialists, have for many years been engaged in working up the zoology of Central America and Mexico. About three years ago it occurred to them that it would add greatly to the interest of their work if the botany could be so far worked

up as to determine the general laws of distribution for comparison with those obtaining in the animal kingdom. After some consultation with Sir J. D. Hooker, and other botanists, it was decided to make the Kew herbarium the basis of an enumeration of all the plants hitherto found in the countries lying between Panama and the territories of the United States. The principal reason that led to this decision was the fact that the vast collections at Kew have recently been carefully studied for Messrs. Bentham and Hooker's "Genera Plantarum," and for various other publications, so that as far as generic types are concerned, Kew herbarium is perfectly available and reliable for the object in view. To this will be added all the species published, but not represented at Kew by named specimens, and the novelties contained in the collections made by the French Scientific Commission. The enumeration will be supplemented by references, localities, altitudes, and everything that will be useful in drawing up an essay on the distribution, &c., of the plants. Although it was impossible to undertake a critical examination of all the species, a considerable number of interesting and apparently new species will be described, and many of them figured, from drawings by Fitch, and coloured drawings done on the spot by Mrs. Salvin.

THE valuable collection made by the Brothers von Schlagintweit during their extensive journeys through India and Thibet is now arranged for exhibition in the royal castle at Nuremberg. It forms one of the most extensive collections ever brought from the East, and possesses objects of rare value, especially for anthropology. Among these might be mentioned the plastic representations of Indian races, numbering 275 specimens, prepared from casts taken from living individuals, and carefully arranged according to castes and races. A large variety of skulls from different regions in India will also prove of no small use to the growing number of students of cranial development.

WE notice the death at Paris of M. J. Fordos, a well-known French chemist, and vice-president of the Paris Chemical Society. His name is chiefly known in connection with researches on thio-acids, and numerous derivatives of sulphur, ligneous colouring matters, estimation of morphium, and several technical subjects, especially the manufacture of cyanides and ferro-cyanides.

PROF. SUESS, the well-known Vienna geologist, is at present traversing Italy with a number of his students, engaged in a geological study of the peninsula. The journey includes Vesuvius and Etna.

THE additions to the Zoological Society's Gardens during the past week include a Prairie Wolf (*Canis latrans*) from the Rocky Mountains, North America, presented by Prof. M. C. Vincent, F.G.S., F.R.G.S.; a Common Jackal (*Canis aureus*) from India, presented by Mr. J. Smith; a Spotted Ichneumon (*Herpestes auropunctatus*) from Nepal, presented by Mr. W. Pyne; a Bronze Fruit Pigeon (*Carpophaga ænea*) from India, presented by Mr. A. H. Jamrach; a White-eared Bulbul (*Pycnonotus leucotis*) from North-West India, presented by Mr. W. Woolner; a Black Saki (*Pithecia satanas*) from the Lower Amazons, deposited.

THE BRITISH ASSOCIATION

REPORTS.

Report of the Committee for Commencing Secular Experiments on the Elasticity of Wires, by J. T. Bottomley.—The Committee have to report that the arrangements for suspending the wires for secular experiments on elasticity are now complete, and that within the last few days two wires, one of palladium and the other of platinum, have been suspended in their places.

An iron tube has been erected in one of the rooms in the tower of the University Buildings in Glasgow. It is 60 feet long, 9 inches wide, and 4½ inches deep from face to back. It is of rectangular section, in lengths of 6 feet, and it is supported by being firmly attached to the heavy outer stone wall of the tower.

At the top of the tube there is a heavy gun-metal plate which is supported independently of the iron tube; and from this plate the wires under examination are to be suspended, as well as additional wires to be used for carrying additional comparison marks. With this arrangement no yielding of the supporting plate that may take place will introduce errors into the results of measurement of the lengths of the wires; for the point of support of the wire carrying comparison marks will experience the same amount of lowering, due to the yielding, as is experienced by the wire to be measured against these marks. The gun-metal plate has been pierced with three rows of holes, through which the wires are to pass. The holes are trumpeted at each end, so as to avoid sharp contact with the wires; and the rows are arranged so that the wires shall hang down in their planes parallel to the face of the tube. It has not yet been decided what is the best way of fixing the upper ends of the wires above the gun-metal plate, or of attaching the weights to their lower ends. No thoroughly satisfactory mode of attachment has yet been found. In the course of experiments to be referred to immediately, which have been carried on at Glasgow on the breaking weight, and the Young's Modulus of Elasticity of the gold, platinum, and palladium wires, which, it is intended, shall be first suspended for examination, several modes of suspension have been tried; but it has not been found possible to make sure of avoiding very considerable weakening of the wire at the points of attachment at the ends.

At the bottom of the iron tube there is a window of plate glass through which the lower parts of the wires can be viewed, and the window can be drawn up so as to allow of the lower parts of the wires being reached.

In front of the window a strong gun-metal table is set up. It is supported independently of the iron tube and of the floor of the room, on iron brackets fixed to the stone wall of the chamber, and is very carefully levelled. On this table a cathetometer is carried, by means of which marks on the wires are to be observed. The cathetometer moves on the table parallel to the planes of the rows of wires. It has the two back feet of the triangular sole plate on which it is supported movable in a V-groove cut in the table, the third foot resting on the plane upper surface. There is also a slot cut in the table through which a screw passes up from below to the sole plate of the cathetometer, and by means of this screw the cathetometer can be clamped in any required place.

The cathetometer is a small instrument which has been constructed by Mr. James White of Glasgow for the purpose of these experiments. The main pillar is one foot high. It is supported on a sole plate having three levelling screws. The telescope or microscope, having cross-fibres, is raised or lowered on this pillar on a proper geometrical slide, and has also a lifting screw in connection with a vernier for giving fine adjustment. The vertical pillar is carefully graduated; and by means of this scale the differences of levels of proper marks put upon the wires are to be determined.

The arrangements have only been completed within the last few days. They require to be carefully tested in several points, and particularly the cathetometer requires careful examination. There is every reason, however, to expect that the work will turn out quite satisfactory. As soon as possible the work of testing will be completed and wires suspended, measured and marked.

During the past year experiments in connection with this investigation have been carried on in the laboratory of the University of Glasgow on the breaking weights and elastic properties of various wires. In the first place the breaking weights and the Young's modulus, or modulus of elasticity for longitudinal pull, have been determined for the gold, platinum, and palladium wire with which it is proposed that the secular experiments on elasticity shall commence. A large number of experiments on the effect of stress, maintained for a considerable time, in altering the breaking weight and the extension under increased stress of various wires, have been carried on. Soft iron wire, steel wire, and tin wire in particular, have been experimented upon, and already some interesting results have been obtained, showing that prolonged application of stress certainly produces a noticeable effect.

Numbers showing the nature of the results already arrived at are appended; but the whole subject still requires much careful examination.

Report of the Committee consisting of James R. Napier, F.R.S., Sir W. Thompson, F.R.S., W. Froude, F.R.S., J. T. Bottomley, and Osborne Reynolds, F.R.S., &c., appointed to investigate the effect of Propellers on the Steering of Vessels.—The Committee are now of opinion that the work for which they were originally brought together has been fully accomplished. The importance of the effect of the reversed screw on the action of the rudder has been fully established, as well as the nature of its effect completely ascertained. Also, for two years, the Committee have urged the results of their work upon the attention of the Admiralty and the various marine boards, and although they regret that, as yet, they have failed to obtain that general recognition of the facts brought to light which their vital importance demands, they consider that this will surely follow, and that as a Committee they can do no more than publish the reports of the trials and the conclusions to which they have been led.

The following is a summary of the conclusions which have been established, and it is interesting to notice that the conclusions drawn by Prof. Reynolds from experiments on models, have been fully confirmed by the experiments on full-sized ships.

Summary of the Results of the Trials of the Effect of the Reversed Screw on the Steering during the Time a Vessel is stopping Herself.

It appears, both from the experiments made by the Committee and from other evidence, that the distance required by a screw steamer to bring herself to rest from full speed by the reversal of her screw, is independent or nearly so of the power of the engines, but depends on the size and build of the ship, and generally lies between four and six times the ship's length. It is to be borne in mind that it is to the behaviour of the ship during this interval that the following remarks apply :—

The main point the Committee have had in view has been to ascertain how far the reversing of the screw, in order to stop a ship, did or did not interfere with the action of the rudder during the interval of stopping, and it is as regards this point that the most important light has been thrown on the question of handling ships. It is found an invariable rule that, during the interval in which a ship is stopping herself by the reversal of her screw, the rudder produces none of its usual effects to turn the ship, but that, under these circumstances, the effect of the rudder, such as it is, is to turn the ship in the opposite direction from that in which she would turn if the screw were going ahead. The magnitude of this reverse effect of the rudder is always feeble, and is different for different ships, and even for the same ship under different conditions of loading.

It also appears from the trials, that, owing to the feeble influence of the rudder over the ship during the interval in which she is stopping, she is then at the mercy of any other influences that may act upon her. Thus the wind, which always exerts an influence to turn the stem (or forward end) of the ship into the wind, but which influence is usually well under control of the rudder, may, when the screw is reversed, become paramount, and cause the ship to turn in a direction the very opposite of that which is desired. Also, the reversed screw will exercise an influence, which increases as the ship's way is diminished, to turn the ship to starboard or port, according as it is right or left-handed; this being particularly the case when the ships are in light draught.

These several influences, the reversed effect of the rudder, the effect of the wind, and the action of the screw, will determine the course the ship takes during the interval of stopping. They may balance, in which case the ship will go straight on: or any one of the three may predominate, and determine the course of the ship.

The utmost effect of these influences when they all act in conjunction, as when the screw is right-handed, the helm starboarded, and the wind on the starboard side is small as compared with the influence of the rudder as it acts when the ship is steaming ahead. In no instance has a ship tried by the Committee been able to turn with the screw reversed on a circle of less than double the radius of that on which she would turn when steaming ahead. So that even if those in charge could govern the direction in which the ship will turn while stopping she turns but slowly, whereas, in point of fact, those in charge have little or no control over this direction, and unless they are exceptionally well acquainted with their ship they will be unable even to predict the direction.

It is easy to see, therefore, that if on approaching danger the screw be reversed, all idea of turning the ship out of the way of the danger must be abandoned. She may turn a little, and those in charge may know in which direction she will turn, or may even, by using the rudder in an inverse manner, be able to influence this direction, but the amount of turning must be small, and the direction very uncertain. The question, therefore, as to the advisability of reversing the screw is simply a question as to whether the danger may be better avoided by stopping or by turning. A ship cannot do both with any certainty.

Which of these two courses it is better to follow must depend on the particular circumstances of each particular case, but the following considerations would appear to show that when the helm is under sufficient command there can seldom be any doubt.

A screw steamship when at full speed requires five lengths, more or less, in which to stop herself; whereas, by using her rudder and steaming on at full speed ahead, she should be able to turn herself through a quadrant without having advanced five lengths in her original direction. That is to say, a ship can turn a circle of not greater radius than four lengths, more or less (see Hankow, Valetta, Barge),[1] so that, even if running at full speed directly on to a straight coast, she should be able to save herself by steaming on ahead and using her rudder after she is too near to save herself by stopping; and any obliquity in the direction of approach or any limit to the breadth of the object ahead is all to the advantage of turning, but not at all to the advantage of stopping.

There is one consideration, however, with regard to the question of stopping or turning which must, according to the present custom, often have weight, although there can be but one opinion as to the viciousness of this custom. This consideration is the utter inability of the officers in charge to make any rapid use of their rudder so long as their engines are kept on ahead. It is no uncommon thing for the largest ships to be steered by as few as two men; and the mere fact of the wheel being so arranged that two men have command of the rudder renders so many turns of the wheel necessary to bring the rudder over that, even where ready help is at hand, it takes a long time to turn the wheel round and round so as to put a large angle on the rudder.

The result is, that it is often one or two minutes after the order is heard before there is any large angle on the rudder, and of course, under these circumstances, it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than fifteen seconds from the time the order is given. Had those in charge of steamships efficient control over their rudders, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger.

Report of the Committee on the best Means of Developing Light from Coal Gas. Read by Mr. T. Wills.—This first part of the report was by Dr. Wallace, and referred to cannel gas, the standard quality of which was assumed to be twenty-six candles. That was the quality delivered by most of the Scottish gas companies, while in London the illuminating power was sixteen candles; in Liverpool, Manchester, and Carlisle, twenty candles; and in most of the other cities in England and Ireland about fourteen candles. The report was largely occupied by a tabulated series of results obtained with different kinds of burners, such as the rat-tail, onion or fish-tail, batwing, and argand, and also the influence of globes of different sizes, shapes, and materials. The report was strongly in favour of cannel rather than common gas, on account of its comparatively small influence on the atmosphere of apartments and the smaller proportion of sulphur it contained. The report also advocated the burning of gas at a comparatively low pressure, and the use of district governors to equalise the pressure in different levels of towns, and of regulators in houses and street lamps, to give the exact pressure calculated to give the best photometric results.

Fourteenth Report of the Committee for Exploring Kent's Cavern, Devonshire. (Abstract.)

The Tortuous Gallery.—When their thirteenth report was drawn, at the end of July, 1877, the Committee had completed the exploration of the "Tortuous Gallery," with the exception of the portions of it termed the "Terminal Chamber" and the "Recess." On entering the Chamber its floor was found to be a complete pavement of blocks of limestone, the removal of which disclosed an almost horizontal bed of the typical breccia—the most ancient deposit yet found in the cavern. It was excavated to the customary depth of four feet, but without reach-

[1] B.A. Reports, 1876-77.

ing its base anywhere. The Chamber measured thirty feet from north to south, from seven to thirteen feet from east to west, and about the same in height. The only objects met with were four pieces of bone and a large lump of oxide of manganese. The Recess yielded twelve teeth of bear and several pieces of bone.

The Undervault.—On the completion of the "Tortuous Gallery" the exploration of an adjacent branch of the cavern, called the "Undervault," was begun. The deposit found in it must be regarded as an uncertain admixture of breccia and the less ancient cave-earth, washed confusedly together. In it were found forty-seven teeth of bear, thirty-three of hyæna, two of fox, numerous bones and fragments of bone, one chert flake, and the greater portion of a large quartzite pebble. Many of the teeth of both bear and hyæna were in jaws and portions of jaws, and some of them were remarkably fine specimens.

The Great Oven.—That branch of the cavern termed the "Great Oven" was partly explored in 1875, but its "Eastern Reach" was then left intact. This reach occupied the Committee from December 18, 1877, to February 15, 1878. It was thirty-four feet long, and varied in width from ten to three feet. The deposits were, in descending order, (1) granular stalagmite, a few inches thick only; (2) cave-earth, also but a few inches thick; (3) crystalline stalagmite, from two to three feet thick; and (4) breccia, the base of which was nowhere reached. The following specimens were exhumed:—Twenty teeth of bear, eight of hyæna, and three of fox, in the cave-earth; one tooth of bear, in the crystalline stalagmite; and fifteen teeth of bear in the breccia. The relics of hyæna were accompanied by a few coprolites.

The High Chamber.—A branch of the cavern, the most remote from the external entrances, has been called the "High Chamber" on account of its suddenly rising several feet above the level of the "Cave of Inscriptions," out of which it opens. Its exploration was commenced in February, 1878, and is still in progress. Up to the end of July it had yielded fifty-three "finds," including eighty-nine teeth of bear, numerous bones and pieces of bone, a chert implement, two chert flakes, and a quartzite pebble, all found in the breccia. There are reasons for believing that this chamber will lead to an entrance of the cavern hitherto unknown.

The president proposed a vote of thanks to Mr. Pengelly. With regard to the quartzite pebble found in the high chamber it appeared to be a block from which a fragment had been separated for some useful purposes. The flint implements are altered, and coated with a white soft substance, some of its original substance having been removed by water.

Prof. Boyd Dawkins said the exploration was a great feat of engineering. He objected to the term quaternary as some times applied to these deposits. The tertiary, quaternary, and pleistocene periods are so closely twined that separation is impossible. He accounted for the arrangement of the animal remains by alternations of climate. In some caves they found generally Arctic animals like the reindeer and glutton associated with animals that were now only found in warm climates, and the probable cause was that there was a great stretch of land over the greater part of the Mediterranean area, from the north of Africa over the region of Europe, until it finally reached Great Britain, and even Ireland. This would afford a means of migration to animals northward and southward, according to the season. This was also shown by the Pyrenees, and so on through Germany, until they arrived at this country. In the Derbyshire caves, Cresswell Cave, for instance, there was a sequence of events very much the same as in this cave. He was now of opinion that the *Machairodus* was a late cave animal, being found in upper cave earth of Cresswell Cave, as well as in Kent's Hole.

Mr. Pengelly was glad to hear Prof. Boyd Dawkins' recantation as to *Machairodus*.

Mr. Plunket read a *Report of the Committee* (secretary, Prof. A. Macalister, M.D.) *on the Fermanagh Caves*, and exhibited a number of animal remains, &c., collected in them.

Prof. Leith Adams said they could not come to any conclusion yet as to the animal remains, for they had not been examined. From what they knew of the extinct animals of these islands, they must come to the conclusion that there was a land connection between England and Ireland.

Prof. Boyd Dawkins said he had looked at the bones, and he found the remains of a domestic hog, shorthorn ox, goat, and red deer—animals which had been used for food—and the bone of a large dog, probably the large deerhound of the sixteenth and seventeenth centuries.

The fourth *Report of the Underground Water Committee* was read by Mr. C. E. De Rance, F.G.S., Assoc. Inst. C.E.—The value of the Government geological maps as a basis for investigation in questions of water-supply was acknowledged; and the areas occupied by permeable formations capable of yielding water in wells sunk in suitable situations was stated to be no less than 26,687 square miles. This, receiving a rainfall averaging thirty inches a year, would yield up to wells not less than six to fifteen inches per annum, or a daily quantity of not less than 240,000 gallons for each square mile of surface, a total quantity far in excess of that required by the population of England or Wales. The great value of the supplies for the towns and districts of the Midland Counties was insisted on because of their purity, and from the unlikelihood of such prospects of water-supply from such sources being stopped by the strong parliamentary opposition which is brought to bear against all large gravitation schemes, whether the water be proposed to be taken from natural lakes, as the Thirlmere for Manchester, or from natural reservoirs as was proposed in the case of Liverpool.

The well borings at Bootle, near Liverpool, of 26 inches diameter just completed for the Liverpool Corporation by Messrs. Mather and Platt, were described as of great interest, the boring having reached a depth of 1,000 feet without reaching the base of the new red sandstone.

The Committee expresses a hope that the boring will be continued, as it may settle several questions not merely of local interest but of national importance: the water-bearing capabilities of the lower beds of the new red sandstone at great depths, the character of the coal-measures which undoubtedly underlie Liverpool, and the depth to them.

Amongst a large number of wells and borings alluded to in the Report was one at Burford, near Witney, in Oxfordshire, where the coal-measures with a coal-seam were found under the ooliths and triassic strata. Then water was also described as being present in the new boring of the Metropolitan Board of Works at Crossness.

Report on the Proposed Kentish Exploration, by R. A. C. Godwin-Austen, F.R.S.—During the past year nothing has been done to warrant an application for any portion of the grant at the disposal of the Kentish boring exploration, but results have been arrived at by private enterprises which indicate the information sought for—whether the palæozoic rocks underlie the chalk formation of the south-east of England as in Belgium and the south of France.

The finding of characteristic upper Devonian fossils in the boring at the corner of Tottenham Court Road and Oxford Street, seems to prove the supposition of the Coal Commission of 1871, that the line of the Thames Valley is that of the course of the palæozoic rocks.

One point remains. In what direction from the end of Tottenham Court Road may the mountain limestone and the coal measures be looked for? That their existence may be certainly inferred is shown by our acquaintance with the physical and geological history of the European area at those early times.

The so-called "Devonian" is but an early stage of the "mountain limestone series," consisting of (1) lower carboniferous or Devonian; (2) carboniferous limestone proper; (3) coal measures.

The Devonian beds, dipping at a high angle, is important; had they been lying flat, it might be inferred that the upper series were denuded, but there is every prospect that these beds may be found underlying the Devonian at some distance from the present bore-hole.

The Tottenham Court Road boring suggests that the Franco-Belgian palæozoic band with which their coal is associated, is continued under London, and within the very narrow limits here assigned (between Oxford Street and the Thames). Considering the vast importance of the discovery of productive coal-measures from the south-east of England westwards, the time has come when the results so nearly arrived at at Tottenham Court Road should be completed. Half the money spent on the Weakden boring at Netherfield would have settled the theoretical question, and even if they were of the same quality as the *Boulonnais* coal, the objections against the quality of that coal have been fully answered.

Major-Gen. Lane Fox, F.R.S., read the *Report of the Earth-*

works Committee: being an Account of Excavations in Cæsar's Camp, Folkestone, and also a paper on *Excavations at Mount Caburn, Lewes, Sussex*.—On Mount Caburn a large number of pits were dug. They were found to contain iron implements and arms entirely of a late Celtic type, and pottery of three descriptions. The fact of these being of a late Celtic type was proved by the getting of the specimens after a considerable amount of digging, and the finding of British tin coins, which were extremely rare, and which were known to be of a late Celtic type. Then excavations were made through the rampart in order to ascertain by means of relics found on the old surface-land what was the date of the first Roman camp. In the upper rampart a quantity of British pottery was found, showing the probability that it was erected at an early time. In the outer rampart pottery of a superior quality was found, by which means it appeared likely that the camp was first erected during the early British period, and was occupied up to the late Celtic period. Subsequently the adjoining camp of Ranscombe was cut through, and Samian pottery was found in the interior, showing that it had been occupied by the Romans; but in the body of the work nothing but British pottery was found, which proves that, though occupied by the Romans, it was not constructed by them. The Ranscombe Camp may, therefore, have been utilised by the Romans, as suggested by Horsfield a long time ago, during an attack upon Mount Caburn. As to Cæsar's Camp, it was ascertained that the whole of the relics found in the body of the several ramparts were of the Norman period, nothing Roman or earlier having been found anywhere.

Report of Committee, consisting of Prof. Harkness and Mr. William Jolly, on the Fossils of the North-West Highlands of Scotland, by Mr. W. Jolly, H.M.I.S., Secretary.—Since the last report one of the most active members of the Committee, the late Dr. James Bryce, has perished in the prosecution of his favourite science. For several years the Committee have explored the great limestone band running from Durness and Loch Eribol to Loch Kishorn, opposite Skye. The fossils discovered were obtained almost entirely from the Durness limestone, fourteen miles east of Cape Wrath, the only other place where they have been obtained in the limestone being at Inchnadamph, or Loch Assynt, in the west of Sutherland. The considerable collection of fossils made was placed in Dr. Bryce's care, but, on his unexpected decease, his collection was found to be more or less scattered and unmarked, and the Committee have as yet been unable to discover the Durness fossils, although careful search and inquiries have been made; they hope to be able to report their discovery to the next meeting.

Through the good offices of several friends, and particularly the Rev. W. C. Grant, a considerable collection has been made from Garveilan, near Cape Wrath, while in the quartzites of Lough Eribol numerous worm or annelid borings have been obtained, and Mr. Mackay has this year discovered hitherto undescribed fossil remains in the same quartzite. It is proposed to have these carefully examined, and to endeavour to ascertain their bearing on the general problem of the place and succession of rocks of the North-West Highlands.

The Committee consider it desirable they should be reappointed, with a grant at their disposal to prosecute their search for fossils, and also to examine and report on those already obtained.

SECTION A.—Mathematical and Physical.

A New Determination of the Number of Electrostatic Units in the Electromagnetic Unit. (Telegram and letter to Sir William Thomson from Prof. W. E. Ayrton.)

The Red Sea, August 3, 1878

From Singapore I sent you the following telegram on July 13.

"Kindly inform British Association that air-condenser measured magnetically and statically gives mean value ratio of these units (29·80) twenty-nine point eight nought ohms.; Foucault's velocity light."

In the autumn of this year I propose communicating a full account of this investigation to the Physical Society or the Society of Telegraph Engineers, or otherwise as you may advise; but I thought that as the British Association Committee had for so long busied itself with the determination of electrical units you might deem the result of this investigation of Mr. Perry and myself worthy of a preliminary notice at the meeting of this Association to be held this year at Dublin. Not being sure that I should arrive in Europe in time to reach Dublin at the commencement of the meeting, although I hope to be present during the last three or four days, I took the liberty of sending you the telegram quoted above.

The result we have obtained for "v" is the more interesting inasmuch as without any bias being given to any one of our experiments, the mean value is identically the same as that obtained by M. Foucault for the velocity of light, whereas all previous determinations of the number of electrostatic units in an electromagnetic unit have led to results differing considerably from Foucault's velocity.

It appeared to Mr. Perry and myself that the method best suited for the accurate determination of v consisted in measuring the capacity of an air-condenser (1), electromagnetically by the swing of the needle of a ballistic galvanometer; and (2) electrostatically by a measurement of the linear dimensions of the condenser, since the equation connecting these capacities

$$s = v^2 S$$

s being the absolute electrostatic capacity,
S ,, electromagnetic ,,

leads to an equation involving only the *square root* of a resistance.

Two difficulties of course presented themselves in this investigation, difficulties that it took us many months to overcome, labouring as we were under the disadvantage of experimenting in a country like Japan. They were—

(1) To obtain a large air-condenser of which the plates had sufficiently true surfaces that the electrostatic capacity could be accurately measured; at any rate when the plates were not nearer than half a centimetre to one another.

(2) To obtain a galvanometric arrangement of sufficient sensibility to measure the small capacity of such an air condenser and sufficiently ballistic that the air damping should be almost inappreciable.

A full description of the condenser we employed (and which had a guard ring, and all the different arrangements we could think of for obtaining accurate results) will accompany the account of the investigation to which I have referred. It is sufficient here to mention that the errors arising from the surfaces of the condenser plates not being true planes were practically eliminated by capacity experiments being made with successive adjustments of the condenser plates, a different set of points in the upper plate being each time brought to the fixed distance from the lower plate.

The arrangement of a ballistic galvanometer to fulfil the two conditions mentioned in (2) was very troublesome. I made several astatic needles none of which satisfied us, and we were beginning to fear my departure from Japan would necessitate the abandoning of the investigation. At last, however, an astatic combination containing forty small magnets (and of which a description will accompany the paper), gave satisfactory results, and I obtained three excellent sets of observations on June 18, June 23, and June 25, when my departure put an end to further investigation. The *mean* values obtained for "v" on each of these three days were—

29·74 ohms, June 18
29·95 ,, ,, 23
29·72 ,, ,, 25

Final mean 29·80

It will be observed that the greatest difference between any one of the three daily means and the final mean, is only about half per cent. The final mean 298·0 million metres per second (and which represents the mean of ninety-eight discharges of the air-condenser) may, I think, be regarded as correct to, at any rate one per cent, and is exactly equal to M. Foucault's velocity of light.

In the astatic combinations I employed prior to June 18 I used eight needles, and weighted the lower set of needles with pieces of brass, so as to give it a barrel shape, but the results were unsatisfactory, as there was either not sufficient delicacy or else too much damping. Consequently all the numbers obtained prior to June 18 have been abandoned. On June 18 were

made the first set of experiments with the forty-magnet astatic combination, the idea of this arrangement being to make an approximately spherical mass of little magnets all slightly separated from one another, and all previously magnetised to saturation. As it would have been too difficult to make this entire sphere all of magnets I finished it off with segments cut from a little wooden sphere. But the half Napierian logarithmic decrement was 0·12095, and we thought this too high. Consequently, in the interval from June 18 to June 23, I took this astatic combination down, and replaced the segments of the wooden sphere by segments of a small leaden hemispherical *shell*. This diminished the half Napierian logarithmic decrement to 0·07825, and with a periodic time of 39·5 seconds I obtained most consistent results. But on the other hand the close agreement of the results obtained on June 18 and on June 25 leads one to conclude that the wooden segments were quite satisfactory, and that replacing them with the leaden shell was unnecessary.

The table at the end of this letter gives the value of the most important constants employed. The battery consisted of 382 perfectly new Daniell's cells in series, and the galvanometer had a resistance of 20,000 ohms. All resistances were compared with a new German silver wire box recently received from Messrs. Elliott, London.

The values obtained for v are (as far as I am aware) up to the present time as follows:—

MM. Weber and Kohlrausch	31·074	ohms.
Sir W. Thomson	28·2	,,
Prof. Clerk Maxwell	28·8	,,
Professors Ayrton and Perry	29·80	,,
Velocity of light, M. Foucault	29·8	,,

During the last twelve months we have been hard at work with the determination of the electromotive force of contact of metals and liquids, using a new apparatus. Some of the results are, we venture to think, most interesting—for instance, the electromotive force of contact of hot and cold mercury, *no other conductors being in contact with either mercury*; the electromotive force of contact of a cold metal and hot mercury, no third conductor being in contact with either, &c. The determination of the electromotive force of contact of the pairs of constituents of Mr. Latimer Clark's constant mercurous sulphate cell was most laborious, and occupied me weeks, in consequence of the difference of potential that exists between the body of the mercurous sulphate paste, and the layer of water that floats on the surface. However, a forlorn hope kept me hard at it, and I am glad to say at last I was successful in getting good results. We have gone over all the old ground that furnished the basis of our former paper, as well as much new ground.

Determination of the Number of Electrostatic Units in an Electromagnetic one.

Date.	Area of condenser plate in square centimetres.	Distance between the plates in centimetres.	Weight of the astatic combination in grammes.	Periodic time in seconds.	Half the Napierian logarithmic decrement.	Mean value of v.	Remarks.
June 18	1324·96	1·024	2·15	25·3	0·12095	29·74	Ninety-eight discharges of the air-condenser.
,, 23	1323·14	0·7728	3·4	39·5	0·07825	29·95	
,, 25	1323·14	0·7728	3·4	42·2	0·081865	29·72	

The distance between the upper condenser plate and the guard ring was slightly increased by diminishing the size of the plate to avoid the possibility of loss of electricity.

June 18.—The lower set of needles was weighted with segments cut from a small wooden sphere.

June 23 and after.—The lower set of needles was weighted with segments cut from a small leaden spherical shell.

Number of magnets in astatic combination 40.

Number of new Daniell's cells in series 382.

General Results of some Recent Experiments upon the Coefficient of Friction between Surfaces moving at High Velocities, by Douglas Galton, C.B., D.C.L., F.R.S., &c.—The author of this paper has been recently engaged in making some experiments upon the coefficient of friction when the surfaces in contact move at high velocities, in connection with the action of brakes in use on railways; and the results which have been arrived at appear to present some interesting features in respect of the laws which govern the coefficient of friction.

The experiments were made to ascertain the friction between the brake blocks and the wheels of a railway carriage.

The levers which move the brake blocks were fitted with dynamometers to show first the pressure which was applied to force the blocks against the wheel, and secondly, the force or tangential strain exerted between the wheel and the block when the latter is pressed against the wheel. The dynamometers used were adaptations of Richards's indicators which act by water pressure which transfers the pressure to cylinders fitted with pistons to which a pencil is attached, so as to register the pressure over a travelling sheet of paper, as is used with steam indicator diagrams. A dynamometer on a similar principle was attached to the draw bar so as to register the force exerted during the experiment in drawing the carriage.

The speed was also recorded on diagrams by means of the Westinghouse speed indicator, which also acts by water pressure and depends for its action on the speed of revolution of the axles.

The carriage or van fitted with the apparatus had two pairs of wheels; one pair of wheels was fitted with brakes whilst the other pair was free. A speed indicator was attached to each pair of wheels, so that the speed of the carriage could be ascertained at any time independently of the speed of the braked wheels.

To check the Westinghouse speed indicator two of Stroudley's speed indicators were also attached to the van, but these do not register automatically. The distribution of the weight of the van between the two pairs of wheels was obtained, as well as the weight of the wheels and axles themselves; and in order to ascertain the weight thrown on the braked wheels during the progress of the experiment, a dynamometer fitted to the springs of the van showed the weight at every moment carried on the unbraked wheels, from which information it was easy to deduce the weight on the braked wheels.

The apparatus was designed by Mr. Westinghouse, and constructed under his supervision by the Brighton Railway Company, through whose assistance these experiments were carried into effect.

The effect of applying the brake to the wheels is twofold. So long as the wheels to which brakes are applied continue to revolve at the rate of rotation due to the forward movement of the train, the effect of the blocks is to create retardation by the friction between the block and the wheel; but when the pressure applied to the blocks causes the friction to exceed the adhesion between the wheels and rail, the rotation of the wheels is arrested, and the wheel becomes fixed and slides on the rail, being held in its fixed position by the brake blocks.

Therefore the experiments give the coefficient of friction—1, between the brake blocks and the wheel, which is equal to

$$\frac{\text{the tangential force}}{\text{the pressure applied}};$$

2, between the wheel and the rail, which is the

$$\frac{\text{friction of the brake blocks}}{\text{weight upon the wheels}}.$$

It has been generally stated that there is no difference in the coefficient of friction observed in the case of bodies at rest, *i.e.*, in a condition of static friction, and the coefficient of friction in the case of moving bodies, *i.e.*, in a condition of kinetic friction; but Mr. Fleeming Jenkin, in his paper read before the Royal Society in April, 1877, upon the friction between surfaces moving at very low speeds, alludes to the fact that in all cases where a difference in the coefficient of friction is observed between static and kinetic friction, the static friction exceeds the kinetic.

Coulomb also points out, in his experiments that in the case of static friction, the coefficient of friction increased with the time during which the bodies had been at rest.

The experiments of Coulomb, Rennie, Morin, and Jenkin, were made with bodies moving at comparatively low velocities.

The following table shows the mean results obtained from a large number of the experiments made with the apparatus above described, upon the action between the cast-iron brake blocks and the wheels fitted with steel tyres:—

	Average.		Coefficient of friction between cast-iron brake blocks and steel tyres of wheels.			
	Miles per hour.	Feet per second.	At commencement of experiment, *e.g.*, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
	60	88	·062	·054	·048	·043
	50	73	·100	·070	·056	—
	45	65	·125	—	—	—
	40	58	·134	·100	·080	—
	30	43	·184	·111	·098	—
	20	29	·205	·175	·128	·070
	10	14	·320	·209	—	—
	Under 5	7	·360	—	—	—
Results by former observers.	Fleeming Jenkin—					
	Steel on steel dry	·0002 to ·0086	·351 mean ·365 max.	—	—	—
	Morin—					
	Iron on iron		·44	—	—	—
	Rennie—					
	At pressure of 1·6 cwt. per square inch.					
	Wrought iron on cast iron		·275	—	—	—
	Steel on cast iron		·300	—	—	—

A limited number of experiments were made with wrought iron blocks upon the steel tyres, a mean of which gave the following result :—

Average.		Coefficient of friction between wrought iron blocks on wheels.			
Miles per hour.	Feet per second.	At commencement of experiment, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
48	—	·110	—	—	—
31	—	·129	·11	·099	—
18	—	·170	—	—	—

The following table shows the result obtained by the sliding of the wheel on the rail—that is, a steel tyre on steel rails :—

Average.		Coefficient of friction between wheel on rail—steel on steel.			
Miles per hour.	Feet per second.	At commencement of experiment, to 3 seconds.	At from 5 to 7 seconds.	At 12 to 16 seconds.	At 24 to 25 seconds.
50	—	·04	—	—	—
45	—	·051	—	—	—
38	—	·057	·044	·044	—
25	—	·080	·074	—	—
15	—	·087	—	—	—
10	—	·110	—	—	—

The general results of these tables show that the coefficient of friction between moving surfaces varies inversely in a ratio dependent upon the velocity at which the surfaces are moving past each other; probably the expression would be of the form of $\frac{a}{b+v}$.

The coefficient of friction, moreover, at these velocities becomes smaller also after the bodies have been in contact for a short time. That is to say, the longer the time the surfaces are in contact, the smaller apparently does the coefficient of friction become. This result appears more marked in the case of cast-iron blocks than of the wheel sliding on the rail. This effect, however, does not appear to be unnatural, as the friction developes heat, and the consequent expansion tends to close up the pores and to make the heated surface a more united surface than the colder surface; besides which it is probable that, in the act of rubbing, small particles may be detached which may act as rollers between the surfaces.

It will also be observed that the coefficient of friction between the cast-iron block and the steel tyre is much larger than that between the steel tyre of the wheel and the rails, which were also generally of steel. As has been above-mentioned, the sliding of the wheel on the rail takes place when the friction of the brake blocks is greater than the adhesion between the wheel and the rail, which is due to the weight upon the wheel. This was found to amount generally to about 24 to 28 per cent. of the weight. The influence which these results have upon brakes for railway trains may be briefly summed up as follows :—

In order to produce a given result at different velocities the pressure applied to the brake blocks must increase in the proportion shown by the coefficient of friction. Thus at fifty miles an hour the pressure required to make one pair of wheels slide on the rail was nearly 27,000 lbs., whilst at twenty miles an hour a pressure of about 10,300 lbs. was found sufficient to obtain the same result. The strain on the draw-bar showed that the retarding force or the tangential strain between the brake blocks and the wheels followed very nearly the same law of variation; that is to say, in order to produce a degree of friction on the wheel at fifty miles an hour which shall exert a retarding force on the train equal to that at twenty miles an hour the pressure applied to the brake blocks at fifty miles an hour must be nearly 2½ times as great as that required at twenty miles an hour, and a still greater pressure is required for higher velocities. Therefore whilst a comparatively low pressure would make the wheel slide at low velocities, it was difficult to obtain any sufficient pressure to make the wheel slide at velocities over sixty miles an hour.

A satisfactory brake, therefore, should be capable of bringing on a very high pressure almost instantaneously, and then the pressure should be gradually reduced as the train comes to rest.

The figures given in the above tables must at present be accepted as only provisional until an accurate mean has been obtained from the diagrams, which are not yet all worked out. But it may be assumed as an axiom that for high velocities a brake is of comparatively small value unless it can bring to bear a high pressure upon the surface of the tyre almost instantaneously, and it should be so constructed that the pressure can be reduced in proportion as the speed of the train is reduced so as to avoid the sliding of the wheels on the rails.

I must add that these experiments were made upon the London, Brighton, and South Coast Railway, who, through their able general manager, Mr. Knight, and locomotive engineer, Mr. Stroudley, gave every assistance in the construction of the van and the running of the train. The apparatus was mainly devised by Mr. Westinghouse and the experiments were carried on under his immediate supervision. The earlier experiments were also made with the assistance of Mr. Horace Darwin.

SECTION B.—Chemical Science.

On a Simplification of Graphic Formulæ, by Oliver J. Lodge, D.Sc.—In the graphic formulæ of a compound the elements were ordinarily represented by their chemical symbols (capital letters), and the connection between the atoms was represented by straight lines joining the letters. Graphic formulæ were of most use in organic chemistry, where the principal compounds consisted only of the elements C, H, O, N, whose atomicities were 4, 1, 2, and 3 or 5 respectively. In any formulæ, therefore, four bonds always radiated from the letter C : N was the meeting place of three or five bonds, according to circumstances; two bonds met at each O, and a single bond terminated at every H. Supposing that the letters were omitted and the bonds joined together, the position of the atoms would still be apparent as the meeting-place of a definite number of bonds, and therefore the letters were unnecessary. The simplification he proposed, then, was the omission of the usual symbols used to denote the atoms, and the joining of the bonds in such a way as clearly to define the atomicities, and therefore the natures of the several atoms. Formulæ so drawn became reduced to a sort of geometrical diagram, and conversely any geometrical curve represented some real or imaginary chemical compound.

Abstract of a Paper on the Action of Heat upon the Selenate of

Ammonium.—Dr. Edward W. Davy, Professor of Forensic Medicine, Royal College of Surgeons, Ireland, read, on behalf of his colleague, Dr. Charles A. Cameron and himself, a paper containing the results of some observations which they had conjointly made on the action of heat upon the selenate of ammonium, from which they have shown that, when that salt is exposed to heat, it first resolves itself into ammonia and an acid selenate of ammonium (a hitherto undescribed salt), and that this latter, on the application of a higher temperature, breaks itself up into water, selenium, selenious anhydride, and nitrogen, thus showing that in the first stage of the decomposition of this selenate by heat it resembles the sulphate of ammonium in furnishing an acid salt under the same circumstances, but that in the separation of selenium in the second stage of the process there is no analogy between the sulphate and selenate of ammonium.

Abstract of a paper *On the Action of Chlorine upon the Nitroprussides*.—Dr. Edmund W. Davy read a paper *On the Action of Chlorine upon the Nitroprussides*, an interesting class of compounds obtained by the action of nitric acid on the soluble ferro- or ferri-cyanides, which were first investigated by Dr. Lyon Playfair, several years ago. Dr. Davy has ascertained that the statements which exist in the different standard works on chemistry as to chlorine having no action on those salts, is incorrect, at least as regards several of the nitroprussides which he has made the subject of investigation, for he has found that some of them are immediately, and others, after long exposure, more or less, acted on by that substance, even when they are excluded from the light. When, however, they are subjected to the combined action of chlorine and the sun's rays, they are soon completely decomposed, the principal products being an oil-like matter, which agrees in its properties with the substance known under the name of chlorocyanic oil, ferric chloride, hydrochloric acid, and a chloride of the metallic base of the salt employed.

The following nitroprussides, viz., those of potassium, sodium, barium, calcium, zinc, iron, and silver, were found to be those decomposed when exposed to the action of chlorine and sunlight; and it is probable that other nitroprussides would be similarly affected. The only one of those salts, however, which the author has observed resisting this action is that of copper, which has remained apparently unaffected after some weeks' exposure to its influence.

On the Spectrum of Chlorochromic Anhydride, by Dr. Johnstone Stoney and Prof. Reynolds.—The authors exhibited and described the spectrum produced by the absorption of the vapour of chlorochromic anhydride. This spectrum is of peculiar interest from its having supplied information as to the duration and character of the motion of the molecules of the gas which produce it. The spectrum consists of lines nearly equally spread, but of various intensities. From the position of the lines, of which 105 have been examined, it has been ascertained that they are all to be referred to one motion in the molecules of the gas of which they are all harmonics or quasi-harmonics, and which, on the supposition that they are harmonics, is repeated 810,000,000,000 every second in each molecule, and from the succession of intensities it may be surmised that this motion is in some way related to that of a particular point in a violin string vibrating under the influence of the bow, viz., a point nearly, but not quite, two-fifths of the string from the one end.

On a New Mdhod of Alkalimetry, by Dr. Louis Siebold, F.C.S.—The plan recommended consisted in the reverse application of Liebig's process for estimating hydrocyanic acid, and was based on the fact that the volumetric determination of an alkaline cyanide by means of silver nitrate was in no wise affected by the presence of free hydrocyanic acid. From the volume of silver solution used the quantity of alkali might be as readily calculated as that of the cyanogen. If the applicability of this process for alkalimetric purposes were confined to the estimation of caustic alkalies nobody would, in Dr. Siebold's opinion, think of using it in preference to the process commonly adopted; but he wished to show that it might with great advantage be applied to the determination of alkaline carbonates. From ·5 to one grain of the potassium or sodium carbonate should be dissolved in about 100 c.c. of distilled water, the solution mixed with an excess of hydrocyanic acid (10 to 20 c.c. of acid of Scheele's strength), and then decinormal solution of silver nitrate added from a burette until a permanent opalescence is produced, the reaction occurring according to the following equation:—

$$K_2CO_3 + 2HCy + AgNO_3 = KAgCy_2 + KNO_3 + CO_2.$$

Whereas under ordinary circumstances hydrocyanic acid was incapable of decomposing alkaline carbonates it effected a complete decomposition in the presence of silver nitrate. The mixture did not require boiling, and the whole operation might be performed within a few minutes. If, after the end of the titration, the mixture were boiled, and the addition of decinormal solution of silver nitrate proceeded with, this time using potassium chromate as an indicator, the volume of silver solution required to insure complete precipitation of the silver cyanide would be exactly equal to that used in the first titration. This second reaction might then, if desired, be used as a check on the determination. In the presence of chloride the volume of silver solution used in the second experiment would be greater than that used in the first, the difference between the two being exactly that required to precipitate the chloride. In this manner a determination of the chloride might be readily combined with that of the alkaline carbonate. The following results were quoted to show the accuracy of the process:—

Pure Potassium Carbonate.

Amount taken.	Amount found.
0·5850	0·5851
0·1670	0·1672
0·8775	0·8779

In mixtures of pure potassium carbonate and sodium chloride—

Amount taken.	Amount found.
K_2CO_3, 0·2000	0·2005
NaCl, 0·0680	0·0683
K_2CO_3, 0·9750	0·9750
NaCl, 0·1825	0·1830

On the Estimation of Mineral Oil or Paraffin Wax when mixed with other Oils or Fat.—William Thomson, F.R.S.E., read a paper on this subject. He said that mixed oils were now often used for lubricating purposes, and a common mixture, composed of mineral oil with some animal, vegetable, or fish oils, was now extensively used, and it was an important point to be able by analysis to determine the amount of mineral oil which such mixtures contained, and as he could find no published process to effect this, he devised, after much work, the following, which he found by repeated tests to give very accurate results:—He boiled some of the sample with an alcoholic solution of caustic soda, which converted all the animal, vegetable, or fish oils into soap. This was then mixed with sand, evaporated to dryness on the steam bath, the residue placed in a bottle washed with petroleum spirit, and distilled at a temperature under 190° F. This dissolves out the mineral oil, leaving the soap insoluble. The spirit is now distilled off from the spirit solution of mineral oil in a large flask, and after thus evaporating off the bulk of the spirit, the concentrated solution is transferred to a smaller flask with a hole blown in its side, into which is fitted a cork carrying a thermometer and glass tube; the thermometer should touch the liquid, going nearly to the bottom of the flask. It is placed on a sand bath and heated at a temperature not exceeding 220° F., and dry air blown into the flask through the tube in the cork, to remove the last trace of spirit, and the residue of mineral oil weighed and calculated on the weight of the original mixed oil taken.

On some Double Salts of Glucinum, by Prof. Emerson Reynolds.—This communication contained an account of some investigations upon some compounds of the rare metal glucinum, and demonstrated by several interesting experiments how the author had succeeded in obtaining the metal in a state of purity by the use of the double fluoride of glucinum and potassium. A second salt, double chloride of platinum, and glucinum, had also been prepared in an exceedingly pure crystalline form. Dr. Reynolds stated that he had also made experiments with a view to determine the specific heat of glucinum, which he fixed at ·642, and this multiplied by the probable atomic weight of the metal—namely, 9·2—gave 5·90, a result fairly in accordance with the law of Dulong and Pettit.

Summary of Investigations in the Pyridine Series, by Dr. W. Ramsay.—These bases, which possess the general formula, $C_nH_{2n-5}N$, are tertiary bases. They form an additive product with iodides of alcohol radicles, of which a good example is C_5H_5N, CH_3I, best named pyridine methyl-iodide, as it resembles a salt in its constitution. They are not attacked by nitrous acid, and the cyanate, when heated, undergoes no molecular change, but merely splits up into the base, and the usual polymer of cyanic acid, cyanuric acid.

Picoline, C_6H_7N, on oxidation, yields a dicarbopyridenic acid, $C_7H_5NO_4$, which, on distillation with soda-lime, decomposes into pyridine, C_5H_5N, and carbonic anhydride, $2CO_2$. It has therefore the structural formula $C_5H_3N(2OOH)_2$. Attempts to prepare lutidine, C_7H_9N, from the aldehyde of that acid, as well as by the reaction

$$C_5H_3N(COOCH_3)_2 = C_7H_9N + 2CO_2,$$

failed, owing, in the first instance, to the small yield of aldehydes, and, in the second, to the total decomposition of the product into pyridine, carbonic anhydride, and carbon.

In spite of the failure of these attempts the author regards it as probable that picoline is methyl pyridine; entidine, dimethyl pyridine, from the following consideration:—The amount of heat evolved in the formation of these bases is probably very high. That heat, added to the amount evolved by the combination of the base with an acid, is likely to be greater than the total number of heat-units evolved by the oxidation of the base; hence these bases are unoxidisable in acid solution. But when oxidised in alkaline solution, the amount of heat evolved by oxidation is supplemented by that arising from the combination of the resulting acid with the alkali, and then exceeds the heat evolved during the formation of the base. The presence of nitrogen therefore gives great stability to the molecule, and prevents the methyl groups from being oxidised to carboxyl-groups, as in the case of toluol, xylol, &c. At least three isomeric acids of the general formula $C_7H_5NO_4$ have been discovered, and it is probable that as many as six are capable of existence. These the author has named α, β, and γ, dicarbopyridenic acids. The α-acid is obtained by oxidising picoline or entidine, and the last two from entidine.

An attempt to pass from furfurol to pyridine by the series of reactions—

$C_5H_4O_2$, Furfurol, → $C_5H_6O_2$, Furfuryl alcohol, → C_5H_5OCl, Furfuryl chloride, → $C_5H_5ONH_2$, Furfuryl amine, → C_5H_5N, Pyridine,

was unsuccessful, owing to the instability of furfuryl chloride.

From the stability of the pyridine group, and the instability of the furfurol group, the author regards it as probable that the constitution of the former is best represented by a closed, and that of the latter by an open, chain.

On Some of the Derivatives of Furfurol, by Dr. W. Ramsay.—It was found impossible to prepare furfuryl chloride by the action of phosphoric chloride, or of hydrochloric acid gas on furfuryl alcohol, $C_5H_6O_2$, owing to a complete decomposition of the organic matter, with separation of carbon.

Furfurine, prepared by heating furfuramide and possessing the same formula, $C_{15}H_{19}N_3O_3$ unites with methyl iodide, forming the hydriodide of methyl-furfurine; this salt, on treatment with ammonia, deposits the base $C_{15}H_{11}(CH_3)N_3O_3$, as a viscous oil, insoluble in water, but soluble in alcohol. The base again unites with methyl iodide, giving the hydriode of dimethyl furfurine, $C_{15}H_{10}(CH_3)_2N_3O_3HI$, which is also decomposable by ammonia with liberation of the base, dimethyl furfurine $C_{15}H_{10}(CH_3)_2N_3O_3$. This base appears also to be capable of union with methyl iodide.

Furfurine, then, appears to be a secondary base containing two atoms of hydrogen replaceable by methyl. Whether more can be replaced the author was unable to decide, as the loss by repetition of the operation was very considerable.

On the Thetines, by E. A. Letts.—Prof. Letts' experiments were undertaken as a sequel to the research of Prof. Crum Brown and himself, on dimethyl thetine and its compounds, and with a view to the thorough investigation of the thetines as a group—the phenomena attending their formation—the action of heat and oxidising agents on them, and the difference in their properties as the series is ascended. Incidentally Prof. Letts has also studied the action of bromacetic acid on certain sulphides of hydrocarbon radicals and the action of bromacetic and iodacetic ethyl ether on sulphide of methyl.

Notes on Aluminium Alcohols, by Dr. Gladstone and Mr. Tribe.—In 1876 the authors described the joint action of aluminium and iodine on alcohol, and two aluminium ethylates resulting from it. They now showed that a similar reaction takes place with methylic alcohol, especially when the aluminium is rendered more powerful by conjunction with deposited platinum; and that an analogous body is still more readily formed from amylic alcohol. These two substitution-products had not yet been prepared in a pure condition, but the authors had succeeded in preparing the butylic compound in a satisfactory manner. This aluminic butylate is a solid body at the ordinary temperature, but melts when heated, and is capable of distillation. It is very soluble in anhydrous ether or benzole, from which it separates on evaporation without crystallising. It is decomposed by water, butylic alcohol and alumina being produced. Its composition was found to be $Al_2(C_4H_9O)_6$. There is also evidence of an intermediate compound, soluble in water, which is probably homologous with the aluminic iodo-ethylate $Al_2I_3(C_2H_5O)_3$.

On the Amounts of Sugar contained in the Nectar of Various Flowers, by Alex. S. Wilson, M.A., B.Sc.—Nectar, the sweet-tasting fluid found within the cups of insect-fertilised flowers is of use to the plant by affording an inducement whereby insects are attracted to visit the flowers. By this means cross-fertilisation is effected, as bees, butterflies, and other insects bring with them pollen from other flowers adhering to their bodies which they deposit on the stigmas. Mr. Darwin has shown experimentally what an additional amount of vigour is thus conferred on the resulting seeds in contrast with the degenerating effect of continuous inbreeding. Very often this sweet fluid is exuded from special glands, but in other cases from portions of the flower that do not seem to have been specially adapted for this purpose. Morphologically nectaries may represent very different structures, but not unfrequently they are of the nature of an aborted organ such as a petal or stamen. It is a disputed point among biologists whether this saccharine matter is a true secretion or simply an excretion of effete matter from vegetable cells—a bi-product of the chemical changes taking place within these cells. Nectar is of course the source whence the bee derives honey, but it also affords sustenance to many different kinds of insects as well as humming-birds. The bright colours of flowers as shown by Sir John Lubbock's experiments serve for the guidance of insects to them, and the odours which they emit fulfil the same end. The markings on a flower's petals, too, always converge towards the nectar. The importance of these guides to insects will be apparent from the following estimations which show how indispensable it is that as little time as possible should be lost by an insect while collecting honey. It must be remembered also that in order to protect the nectar from rain it is usually contained in the least accessible part of the flower. The formation of nectar is observed to take place most freely in hot weather, so great, however, is the economy of the plant that it is only formed at the time when insects' visits would be beneficial, *i.e.*, when the anthers are shedding their pollen or when the stigma is mature. Biologists believe that the visits of bees, butterflies, and other insects have in past time exercised an important influence in modifying the size, shape, colour, &c., of flowers. The following determinations are of interest as showing to what extent this action goes on and as a help towards ascertaining the value of this factor:—

Sugar in Flowers.

		Total.	Fruit.	Cane.
1. Fuchsia, per flower	mmg.	7·59	1·69	5·9
2. Everlasting Pea, per flower...	,,	9·93	8·33	1·60
3. Vetch, per raceme	,,	3·16	3·15	·01
4. ,, single flower ...	,,	·158	·158	--
5. Red Clover, per head... ...	,,	7·93	5·95	1·98
6. ,, ,, floret... ...	,,	·132	·099	·033
7. Monkshood, per flower ...	,,	6·41	4·63	1·78
8. *Claytonia alsoides*, per flower	,,	·413	·175	·238

Approximately, then, 100 heads of clover yield ·8 grm. sugar or 125 give 1 grm. or 125,000 1 kilo sugar, and as each head contains about sixty florets it follows that 7,500,000 distinct flower tubes must be sucked in order to obtain 1 kilogrm. sugar. Now as honey roughly contains 75 per cent. of sugar, 1 kilo. is equivalent to 5,600,000 flowers in round numbers, or say two-and-a-half millions of visits for 1 lb. of honey. Another point worthy of note in these results is the occurrence of what appears to be cane-sugar, and that in the case of fuchsia in the proportion of three-fourths of the whole. This is remarkable, as honey is usually supposed to contain no cane-sugar, its presence being generally held as certain evidence of adulteration. The question therefore arises whether this change, which occurs while the sugar is in the bees' possession, be due to the action of juices with which it comes in contact while in the honey-bag, or whether on account of the acid reaction of nectar it may not take place spontaneously.

Notes on Waters from the Severn Tunnel Springs, by W. Lant Carpenter.—The plans for the construction of this tunnel had

been described by Mr. Charles Richardson, its engineer, to Section G, at the Bristol meeting in 1875. In June, 1878, the trial heading had been driven more than half-way under the Severn, and the most difficult part had been overcome. In the opinion of the engineer, the water in the tunnel springs—one of which had yielded 300 gallons per minute—was not Severn water, but derived from springs in the pennant and other strata, and the salt from the occasional beds of salt in the Bristol coal-field. The author had analysed water from four springs, and compared the results with those from Severn water at various times of the tide, and with water from wells on the English side sunk forty feet through the alluvium. The results were arranged under the heads of specific gravity, total solid residue, and total chlorine per cent. The author found it difficult to avoid the conclusion that the water in the springs was chiefly Severn water.

SECTION C.—GEOLOGY.

The Geological Relations of the Atmosphere, by T. Sterry Hunt, LL.D., F.R.S.—The author began by noticing the inquiries of Ebelmen into the decomposition of rocks through the influence of the atmosphere, resulting in the fixation of carbonic acid and oxygen, and discussed the question at length, with arithmetical data. He inquired farther into the fixing of carbon from the air by vegetation, with liberation at the same time of oxygen both from carbonic acid and from the decomposed water, the hydrogen of which, with carbon, forms the bituminous coals and petroleums. It was shown that the carbonic acid absorbed in the process of rock-decay during the long geologic ages, and now represented in the form of carbonates in the earth's crust, must have equalled, probably, two hundred times the entire volume of the present atmosphere of our earth. This amount could not of course exist at any one time in the air; it would, at ordinary temperatures, be liquefied at the earth's surface. Whence came this vast quantity of carbonic acid, which must have been supplied through the ages? The hypothesis of Elie de Beaumont, who supposed a reservoir of carbonic acid stored up in the liquid interior of the planet, was discussed and dismissed. The gas now evolved from the earth's crust from volcanic and other vents was probably of secondary origin, and due to carbonates previously formed at the surface.

The solution of the problem offered by the author is based upon the conception that our atmosphere is not terrestrial, but cosmical, being a universal medium diffused throughout all space, but condensed around the various centres of attraction in amounts proportioned to their mass and temperature, the waters of the ocean themselves belonging to this universal atmosphere. Such being the case, any change in the atmospheric envelope of any globe, whether by the absorption or the disengagement of any gas or vapour, would, by the laws of diffusion and static equilibrium, be felt everywhere throughout the universe, and the fixation of carbonic acid at the surface of our planet would not only bring in a supply of this gas from the worlds beyond, but by reducing the total amount of it in the universal atmosphere, diminish the barometric pressure at the surface of our own and of all other worlds.

This conception of a cosmical atmosphere, of which our own forms a part, is not new, but was put forth by Sir William R. Grove in 1843, and is developed in the very learned and ingenious work of Mr. Mattieu Williams, on "The Fuel of the Sun," and has lately been noticed by Dr. P. M. Duncan in its geological bearings. Ebelmen, in 1845, pointed out that the greater weight of an atmosphere charged with carbonic acid would increase the temperature due to solar radiation at the earth's surface, and greatly modify atmospheric phenomena.

Tyndall, by his subsequent researches on radiation, showed that certain gases, in amount too small to affect considerably the barometric pressure, might influence powerfully climatic conditions, and suggested that in the former presence in the atmosphere of moderate quantities of a gas like carbonic acid, might be found a solution of the problem of the climates of former geologic ages. According to the author, the amount of this gas, which, since the advent of life on our earth, has been subtracted from the universal atmosphere, although it may not have sufficed to diminish by more than a small fraction the pressure at the earth's surface, would account for all the conditions of geological history so far as temperature and climate are concerned.

He maintains that while we have evidence of a warm or sub-tropical climate prevailing over the Arctic regions from the carboniferous down to lower cretaceous times, and a gradual refrigeration up to the temperate climate of the miocene age, we had for the first time in the pliocene age the evidence of Arctic cold, which, with some variations, has continued until now. Since that date geographical variations have caused, and may again cause local climatic changes of considerable magnitude. But no such changes could permit the existence over continental areas within the Arctic circle, of such tropical vegetation as we know to have once flourished there. Geographical changes, as J. F. Campbell, Dawson and others have so well pointed out, might lift large areas into the region of perpetual frost, and thus give rise to local glacial phenomena, and may, moreover, account for considerable local climatic variations at the sea-level since the pliocene age. We cannot, however, account in this way for the warmer climates of previous ages, but must seek for their cause in the former constitution of the atmosphere.

Touching the suggestion that former climatic changes were due to a displacement of the earth's axis of rotation, the author expressed the opinion that it is irreconcilable with the fact long ago insisted upon by him that "the direction of the Arctic currents, which are guided by the earth's rotation, appears, from the distribution of marine sediments, to have been the same since very early periods." Dawson has reinforced this argument by recalling the fact that the southward migration of successive floras shows, in like manner, that from the Devonian age the general courses of oceanic currents, and consequently the position of the earth's axis, have not changed.

On the Filtration of Sea-water through Triassic Sandstone, by Isaac Roberts, F.G.S.—Mr. Roberts stated that he was led to investigate the effects produced on sea-water by filtration, in consequence of the constantly-increasing salinity of the water drawn from several wells in Liverpool which are sunk below the sea-level in the Bunter sandstones of that locality. He found that one of the wells, which he selected as the type of the rest, yielded water which increased in salinity at the rate of 4·91 to 5·81 per cent. annually, and inferred that the sandstone rock had the power of removing salts out of sea-water. To prove this he filtered sea-water through blocks of the sandstone, and found the inference to be greatly borne out by the results of his experiments. Two cubic feet of the stone removed from the first filtrate of 3½ fluid ounces of the water 80·8 per cent. of the salts held in solution, and each measured quantity of four ounces, which were afterwards filtered through, regularly showed an increase of the salts in solution, until 93½ fluid ounces had filtered through the stones. Then these ceased to be operative as filters, and the waters passed through unchanged. After allowing the stones to dry he passed spring water through them, and found that the salts which they had taken up were again removed and washed out, thereby showing the action to be mechanical.

New Geological Map of India.—Mr. W. Ball, M.A., presented and explained a new geological map of India which will shortly be published, with a manual. He mentioned that there were 8,000 square miles of coal measures in India. It was of an inferior character, and was unfortunately in the most inaccessible part of the country.

Mr. W. H. Baily, M.R.I.A., F.G.S., read a paper *On some Additional Labyrinthodont Amphibia and Fish from the Coals of Jarrow Colliery, near Castlecomer, County Kilkenny.*—These fossil remains, which were found below a bed of coal three feet in thickness, were all impressed on the true coal, and had, in fact, turned into carbon. One of them, which was almost perfect—*Megalichthys hibberti*—was three feet seven inches in length. This one locality had yielded a larger number of these fossils than all the other coal-fields in Europe.

On the Ancient Volcanic District of Slieve Gullion, by Joseph Nolan, M.R.I.A., Geological Survey of Ireland.—Slieve Gullion is a mountain situated some few miles north of Dundalk, west of the hilly country lying between the bays of Dundalk and Carlingford. The rocks which mainly compose it are of a plutonic character, consisting of dolerites and elvanites, and have been erupted through granite of lower Silurian age, probably about the close of the palæozoic epoch. On the west and south of the mountain the elvanite forms a dyke-like ridge when it changes from a granitoid rock to a felstone porphyry. Simultaneously with this change, suggesting conditions of less intense heat and pressure a remarkable fragmentary rock makes its appearance. It is here almost altogether composed

of granite fragments, farther south a mixture of granite and slate, and at the south-east almost exclusively of slate *débris*. At all these places it is so intimately associated with the porphyry that any demarcation between them is impossible, and there can be no doubt that it is due to the broken and disintegrated crust, shattered by the force of the volcanic explosions. That the eruption differed from volcanic explosions of the normal character is evident from the absence of fragments of lava or scoriæ. It was in fact entirely aëriform, the volcanic activity having evidently been subdued by the sudden dissipation of the elastic gases in that part of the dyke which alone reached the surface, aided by the immense weight of displaced materials falling back into the gulf.

The president remarked that the paper opened up several points of interest, especially in illustrating the passage of granitic into volcanic rocks; and in bearing out the views of Prof. Judd on these points, giving undoubted proof of the change from granitoid rock into one by which its protrusion has produced mechanical accompaniments.

On Some Fossils from the Northampton Sands, by John Evans, D.C.L., LL.D., F.R.S.—These fossils from the ironstone beds of Durton, near Northampton, are casts of lithodomous borings originally made in lumps of coral, impressions of which they still bear on their outer surface. In the interior the presence of the shells is still to be traced. Their history appears to be that the cavities were first filled with limonite, subsequently connected with carbonate of iron, and eventually hematite. Last of all the inclosing coral has been entirely removed by these infiltrations of water charged with carbonic acid. This last process has probably taken place since the emergence of the beds from below the sea-level. The Northampton Sands have been fully described by Mr. Samuel Sharp, F.G.S., in the *Quarterly Journal* of the Geological Society.

A paper was read by Mr. H. W. Baily, F.G.S., *on a New Starfish from Lower Silurian Caradoc Strata, Co. Wexford, and some New Carboniferous Limestone Mollusca from the County of Limerick*.—The starfish, of which a number of very perfect specimens had been collected through the exertions of Mr. H. Kinahan, during the progress of the Geological Survey, was named by Mr. Baily *Palasterina kinahana*. The carboniferous limestone fossils were all molluscan shells, principally cephalopods, belonging to *Nautilus goniatites* and *Orthoceratites*, with a few bivalves and univalves belonging to *Cardiomorpha macrocheilus*, &c.

On the Saurians of the Dakota Cretaceous Rocks of Colorado, by Prof. E. D. Cope.—This paper was illustrated by crayon-drawings of the bones, of the natural size, which well illustrated the gigantic proportions of the several species. The first described was the *Camarasaurus supremus*, Cope, of one individual of which a large number of bones have been found. The vertebræ are opisthocœlous, and their centres are hollow, with the internal cavity divided into two chambers by a septum. The caudal vertebræ and limbs are solid, or nearly so, and the neck is long. The scapula is enormous, measuring 5½ feet in elevation, so that the fore limb was probably as long as or longer than the hind limb. There are four sacral vertebræ and the femur is six feet in length. The elevation to the top of the head was probably 26–28 feet. The description of three species of *Amphicælias* followed. The type *A. altus*, Cope, presents the same kind of chambers of the vertebral cerebrum as *Camarasaurus*, but the articular extremities are both concave. Both genera possess a new articular element, called by Prof. Cope a hyposphen. This is the reverse of the zygosphen, standing below the postzygapophysis, and looking upwards and outwards. The neural spine of *Amphicælias* is anteroposteriorly placed, that of *Camarasaurus* transversely, and in the latter, in the dorsal region, it is divided to the base, and the halves separated. *A. altus* was as large as *C. supremus*, the femur measuring six feet and the dorsal vertebra 3·5 feet elevation.

Prof. Cope then indulged in some reflections as to the habits of the genera so remarkable for their thin-walled dorsal and cervical vertebræ. He was of the opinion that the caverns were not filled with cartilage but with diverticula from the lungs or other air-cavities. Prof. Cope advanced the hypothesis that the species of *Camarasaurus* and *Amphicælias* were dwellers in water of sometimes considerable depth, where they walked on the bottom, and browsed on the algæ and sometimes aërial vegetation growing on the coast. The long fore-limbs and long neck of *Camarasaurus* were further evidence that the animal reached upwards for food, as the giraffe, or for air when submerged.

Prof. Cope concluded by remarking that the Dakota epoch had been referred to the middle of the European cretaceous series on the evidence of the mollusca and plants, but now the vertebrata parallelise it with some portion of the Jurassic series in the wide sense.

Prof. Hull, F.R.S., gave a sketch of the geology of the environs of Dublin.

On the Progress of the Geological Survey of Ireland, by Prof. Hull, M.A., F.R.S., Director (communicated with the sanction of the Director-General).—The author gave a short account of the origin and progress of the Geological Survey from its commencement in 1832, under the late Gen. Portlock, R.E., down to the present day, stating that the whole country south of a line drawn roughly from Larne on the coast of Antrim to Sligo had been surveyed, while 160 sheets of the geological map, on a scale of one inch to the statute mile, had been published.

Along with the maps there had been issued seventy-eight separate explanatory memoirs, describing the structure and palæontology of 126 sheets. It had also been found necessary to revise the geology of the Leicester and Tipperary coal-fields, the carboniferous trap-rocks of county Limerick, and the south-east portion of the country, including parts of Wicklow and Wexford. The coal-fields of the north of Ireland had also been surveyed, and published in the maps both of the "6-inch" and "1-inch" scales; and it was also intended that the districts of county Antrim containing the pisolitic iron ores should be illustrated by maps on both these scales. The district still remaining to be examined includes the greater portion of Donegal, Tyrone, Sligo, Fermanagh, and Antrim.

On the conclusion of Prof. Hull's paper *On the Progress of the Geological Survey*, the President said they must all congratulate the chief of the Geological Survey in Ireland on the progress that had been made, but he hoped that however long he might live in the occupation of that high post, the survey would never be absolutely completed. It was quite right to re-examine surveys that had been made; for, from time to time discoveries were made which threw a further light on the geological views of the district, and which must be taken into account before an absolutely complete survey could exist. Therefore, though the survey had progressed so much, still he hoped it would be long before it was completed. He wished to say one word which appeared to him of importance, not only to the Geological Survey of Ireland, but of England also. The object of these surveys was to make the public at large thoroughly acquainted with the geology of the country in which they resided. Maps were carefully drawn and memoirs published from time to time in illustration of the maps; but, unfortunately, so far as the diffusion of knowledge was concerned at the present time, not owing to any prohibition of the Geological Survey, but owing to some mistaken view on the part of the Treasury, prohibitive prices were placed upon the geological memoirs. He had seen small pamphlets priced at 16*s.* or 17*s.*, though these pamphlets were printed and published at the public expense for the benefit of the public. He held in his hand a very small pamphlet, which was published at 9*s.* He did not think a false economy of this kind ought to be suffered to go on without a protest on behalf of those who were interested in geological progress. He therefore felt it right to make these remarks, in the hope that possibly this Association or some other learned bodies might take steps to bring this matter under the consideration of the Treasury, and point out how with the one hand they were lavishly spending money for the advancement of geological knowledge, and with the other withholding it from the public.

Dr. Sterry Hunt said that in the State of Pennsylvania the reports and memoirs were printed and stereotyped, and then offered to the public for the mere cost of printing and paper. The memoir on the table priced at 9*s.* would be sold there for 2*s.*

Mr. Tiddeman said, with respect to the prices of the geological publications, he should be sorry any one should think that the Survey was responsible. It could not be more disagreeable to any than to the Survey themselves to find that their labour was almost entirely thrown away by reason of the high prices of the publications when they came out in the miserable manner in which they were produced, and which would almost disgrace a fifth-rate publishing firm.

On the Influence that Microscopic Vegetable Organisms had in the Production of some Hydrated Iron Ores, by M. Alphonse Gages, M.R.I.A.—In the tanks of the Royal College of Science, Dublin, there are considerable deposits of hydrated peroxides of

iron formed by the agency of Penecilliæ, which accumulate in large sponge-like masses so as to intercept the flow of the water. The author points out the connection between such deposits and iron-ore formations.

Notes on some New Fossils "Eribollia Mackayi," from the Quartzites of Loch Eribol and other Parts of the Western Highlands of Scotland, by James Nicol, F.R.S.E. (with photograph of the rock).—The fossils were discovered by Mr. Donald Mackay, early this year, in the Assynt quartzites of Lough Eribol, lying below the Durness limestones, in which fossils have been more abundantly found. The surface of the rock shows the ends of more than a dozen bodies, more or less rounded in form, running down into the interior of the stone, and tapering to a point. They show a central core with an outer wall around it. There is no doubt they are organic. The author rather inclines to regard them as corals approximating at least in form to *Cyathophyllidæ*. Thin slices might perhaps decide this question, but meanwhile the author has named them *Eribollia Mackayi*, indicating the locality and the finder. These and other fossils give the author no reason to reverse the relations of the strata in the north-west Highlands held by him, but much to confirm them.

Concerning the Extent of Geological Time, by Rev. M. A. Close, F.G.S.—Since geology has her own strong and unrefuted arguments for the great extent of geological time, it is not logically necessary for her to do more than show, if it can be shown, that the physical arguments for the very inconvenient restrictions thereof rest upon still unproved assumptions. The argument from the rate of cooling of the earth seems to have been satisfactorily shown by Mr. T. Mellard Reade to be quite inconclusive. The argument from the probable duration of the sun's radiation of heat assumes, *inter alia*, that the original nebula from which the solar system was formed was cold, and also that the unit of gravitation relatively to the mass of that system has been constant from the time when that mass began to fall together, and throughout the enormous interstellar distance which has doubtless been traversed by it since that time. Dr. Croll's suggestion in answer to the former of these assumptions is logically sufficient as a reply to the whole of this argument. Nevertheless it may be added, as to the latter assumption, that those physicists who have entered upon certain speculations as to the cause of gravitation, cannot deny that it is perfectly credible, and even probable, that gravitation is not an essential accompaniment of matter, and that the unit of gravitation may not be constant throughout all time and space. The arguments from the earth's figure in connection with the retardation of her rotation by the ocean tide depends on the doctrine of the steel-rigidity of the earth taken all together, as do also the calculations of various writers on subjects which bear in different ways on the present one. However, Sir W. Thomson himself has greatly weakened the support of this doctrine. But geology (as regards the matter in hand) is not concerned to question it, although it is, at first sight, a difficulty. The results obtained by the Tide Committee of the Association point to the conclusion that there is an 18·6 year-tide in the body of the earth depending on the revolution of the moon's nodes, and that the rigidity of the earth, even if it be in one sense as high as that of steel, is yet a *viscous* rigidity, by which she may yield almost indefinitely to sufficiently long-continued straining forces. Other considerations confirm this latter position. This 18·6 year-tide, whether resulting from such *viscosity* proper or from plasticity of a different kind, must cause a variation in the earth's rate of rotation during the same period. This variation would probably be desirable if looked for by the astronomers, who would confer a boon on the geologists by endeavouring to detect it. Dr. John Evans' suggestion of the possible considerable mobility of the axis of rotation relatively to the body of the earth, bears in certain ways on the present question; the mechanical objection to it, already greatly weakened by the Rev. O. Fisher, might be quite removed by the investigation suggested.

On some New Pre-Cambrian Areas in Wales, by Henry Hicks, M.D., F.G.S.—During some recent researches in Wales the author has been able to add many new areas to the pre-Cambrian rocks already described. In these examinations he has been assisted at different times by Prof. Torrel, of Stockholm, Prof. McKenny Hughes, Mr. Tawney, F.G.S., and Dr. Sterry Hunt, of Montreal. The additional areas to be now added to those previously known are:—

1. Some cupriferous schists with their associated greenstone bands (the so-called intrusive greenstone of the Geological Survey) to the north of Dolgelly, and including a great portion of Robel Tawr.

2. Masses of granitoid rocks, porphyries, and greenstone breccias, in the neighbourhood of Pwllheli.

3. The porphyries and granitoid rocks forming Myaydd Mynytho, and extending in a northerly direction towards Nevin, including also Nevin mountain and the porphyries and greenstone breccias to the north-east of Boducan.

4. The Bhos Hirwain syenite and the so-called altered Cambrian beds to the west of that mass in Caernarvonshire, and also Bardsey Island.

5. The granitoid rocks, felstones, and porphyries, forming the Rivals (yr Eifl) range of mountains.

6. The so-called altered Cambrian rocks to the west of the Penygroes porphyry.

7. The so-called intrusive granite in Anglesea, and the whole of the area marked as altered Cambrian in that island. In addition to these he has also extended some of the areas and defined more clearly the order of superposition of these rocks in Pembrokeshire. In North Wales, as in South Wales, he found that the pre-Cambrian rocks resolved themselves into three well-marked and very distinct types, and that these indicated separate formations, each of which, on careful examination, and when found in juxtaposition, proved to be unconformable to the other. At St. David's the granitoid rocks occur at the base, and, resting unconformably upon these, are found the quartz-felsites. These are again succeeded unconformably by the agglomerates, breccias, greenstone bands, and schists of the Pebidian group.

In North Wales this was also exactly the order in which the various rocks were found to succeed each other, but the middle or quartz-felsite group was found more largely developed in Caernarvonshire.

As this middle group had not previously been separated under a distinguishing name, the author now proposed to adopt for it the name *Arvonian*, from the Roman name *Arvonia*, and from which the present name of Carnarvon is derived. So many of the large ridges and lofty mountains of Carnarvonshire are composed of these felsitic rocks that it appeared to the author and his friends that this name would be very appropriate for the formation. The distinguishing characters most marked in these three pre-Cambrian formations may be briefly summed up as follows:—

1. *Dimetian:* Granitoid gneiss rocks.

2. *Arvonian:* Quartz, felsites, and porphyries (Halleflinta of Torrel; petro-silex rocks, Hunt).

3. *Pebidian:* Green and purple agglomerates and breccias, green chloritic schists, with massive greenstone bands, talcose schists, &c.

In these formations the bedding is usually easily recognised, but at present the actual stratigraphical thickness cannot be correctly estimated. It is perfectly clear, however, from the sections exposed, that each must have a vertical thickness of many thousand feet. That they have a very extended geological distribution over the British islands is also daily becoming more and more evident.

On the Metamorphic and Intrusive Rocks of Tyrone, by Joseph Nolan, M.R.I.A., &c., of the Geological Survey of Ireland.—The rocks described in this paper occupy the central parts of county Tyrone extending from Omagh eastwards and north-eastwards towards Slieve Gullion. They consist for the most part of an amorphous green hornblendic rock, in the midst of which is a wide lenticular tract of micaceous gneiss and schist. The author shows that these two classes pass gradually into each other, and that even among the amorphous hornblendic rocks traces of schistose structure can generally be observed, while local transitions into schists frequently occur. Gradations into more crystalline rocks were also noted and described, those of a hornblendic character passing into a felspathic variety in which little or no hornblende occurs, while quartz and orthoclase are developed, so that a coarse quartz porphyry is produced, passing ultimately into granite.

It was also shown that some of the granite was intrusive during the period of the old red sandstone, a new fact in Irish geology. Its association with metamorphic rocks, probably of lower Silurian age, was explained on the hypothesis that the intrusive granite was due to re-metamorphism of the later period, so that portions of the already crystalline rocks were completely fused and became irruptive. That metamorphic action in this district continued up to and even after the old red

sandstone age seems to have been the opinion of the late Gen. Portlock, who in his geological report on Londonderry, with parts of Tyrone, &c., has described these rocks and their relations to each other at considerable length. He does not seem to have considered the granite to be intrusive, but merely a metamorphosed condition of what we now call the lower carboniferous sandstone, which was then classed with those of the old red formation.

On the Cervus Megaceros, by W. Williams.

On the Occurrence of Certain Fish Remains in the Coal Measures, and the Evidence of the Fresh-water Origin of the Coal Measures, by James W. Davis, F.G.S.—These occur in a bed of cannel coal, and more particularly in the impure cannel above and below it, in the district of Morley and Adwalton, near Leeds. They consist of both Elasmobranchs and Ganoids, but by far the most common are *Cœlacanthus lepturus*. The fresh-water nature of the cannel coal and the internal anatomy of the Cœlacanthus, together with occurrence of Labyrinthodonts, lead to the conclusion that the strata were of subaqueous, and probably fresh-water origin.

On the Age of the Crystalline Rocks of Donegal, by Prof. W. King, D.Sc.—The author had succeeded in obtaining some true fossils in portions of the Innishowen limestone that have scarcely undergone any change. He had not had time to examine them as closely as he would have wished, but they appeared to be identical with Caradoc Bryozoon from the Desertcreat schists of Tyrone, which Portlock has called *Ptilodictya dicotoma*. This was the first example, as far as he could ascertain, of an undoubted fossil having been detected in these limestones. The fact may be taken as evidence that these deposits and their associated argillaceous and siliceous masses are of the lower Silurian age, and it seemed highly probable that the more intensely metamorphosed rocks in the north-west division of Donegal belonged to the same geological period.

On the Cervus Megaceros, by W. Williams.—The author considered that in some excavations he had made at Ballyhetagh Bog, near Dublin, he had met with evidence which led him to differ from former views as to the time of these animals' existence. He asserted that the clay on which the remains rested is the *lower boulder clay*. He considered the animal lived during the middle glacial period, and was killed off by the cold of the *uppper boulder clay* period.

Prof. Leith Adams, F.R.S., questioned the accuracy of Mr. Williams' views. There was no evidence for such a division of glacial beds in the locality in question, as Mr. Williams had mentioned, nor was there any proof that the animals had been destroyed by the intervention of a cold period. In England there is authentic evidence of the co-existence of this animal with man.

After the discussion on the *Cervus megaceros*, the president submitted the following lines:—

> "Small comfort to the stag that's mired,
> To think that in long distant ages
> He'll be dug out to be admired,
> And have his life discussed by sages."

which Mr. Pengelly capped as follows:—

> "Yet had he known their fearful puzzle,
> How far from truth each sage would be,
> Methinks he'd rear his cervine muzzle,
> And sceat the future Section C."

SECTION D.—Biology.

Department of Anthropology.

Miss A. W. Buckland read a paper *On the Prehistoric Monuments of Cornwall compared with those in Ireland.*—In the course of her paper Miss Buckland said that the prehistoric monuments of Cornwall, believed by archæologists to be the work of the same race as those of Ireland, presented, in the midst of strong resemblance, certain points of difference which deserved the attention not only of archæologists but of ethnologists. In both countries they consisted of tumuli, including chambered barrows and giants' graves, monoliths or menhirs, circles, cromlechs or dolmens, and holed stones, all probably sepulchral and hut circles, cliff castles, curious caves and crosses, whilst in Ireland they found in addition earthworks called raths, and round towers. Long barrows, which were looked upon as the most ancient of burial places belonging to the stone age, were wanting in both countries, hence we may infer that the people who then existed in England and Scotland never inhabited Cornwall and Ireland, where the earliest barrows seem to belong to the bronze age, the mode of interment in Cornwall being chiefly by cremation; but these tumuli may not represent the earliest tombs in these countries.

Mr. W. J. Knowles read a paper *On Flint Factories at Portstewart and Elsewhere in the North of Ireland.*—The paper contained a further account of a find of flint implements found in sandhills at Portstewart, county Londonderry, near the mouth of the River Bann, which consisted of a large quantity of scrapers, some arrow-heads, bone implements, hammerstones, and flakes, with shells, broken bones, and pottery. These were found in hollows amongst the sand, but were supposed to have dropped from black layers on the sides of the pits as the sand was removed by the wind. The black layer represents the ancient surface, and similar objects have been dug out of it. The only new find since the subject was last brought before the British Association was some porous lava and flakes of obsidian. These substances are not found native, as far as the author could learn, and he believed they must have floated a distance, probably from the West Indies. He also described other places which he had explored in the neighbourhood of Castlerock, county Londonderry, and Ballintoy, county Antrim, where similar remains were found, and the same kind of black layers are to be seen. The black layers, when not destroyed or uncovered by denudation, are covered up with sand from ten to twenty feet in thickness, which is protected on the top, but is gradually wearing away where exposed at the sides. The animal remains were submitted to Prof. A. Leith Adams, of the Royal College of Science, and he decided that they contained man, horse, ox, hog, dog or wolf, fox, and deer. He also described objects from Larne, some of which were found nine feet from the surface, and he exhibited a photograph of a mammoth's tooth, found in a small delta-like field near Larne, which is now in the possession of the Rev. Dr. Grainger, M.R.I.A., of Broughshane, county Antrim, and flakes and rude implements from the same field. He also showed and described a series of rude implements thick at the one end, for holding in the hand, and pointed at the other, of the palæolithic form. Although there were no animal remains found with them they were taken from the diatomaceous deposit below the peat, where remains of the Irish elk are usually found, and the author drew attention to a statement of Dr. John Evans in his "Stone Implements of Great Britain," where, on comparing the implements found in the caves and those in the old river drifts, he says—"the large-pointed implements are mostly found in the latter;" and gives as his reason for their being found mainly there that they were probably used "for out-of-door purposes." The author states that it was strange that his large implements of similar form were found mainly in the bed of a river, and suggested that the rivers and that form of implement had probably some connection, and that they were not used for "out-of-door purposes" only. Reference was next made to the age of the implements found in Ireland, about which he said authors were not agreed, and the author again quoted from Evans' "Stone Implements," and showed that the descriptions of many of the palæolithic implements in that work would apply to the Irish ones, and concluded that we must either carry man farther back than the so-called neolithic age, or give up some of our theories regarding the distinguishing characteristics of the palæolithic and neolithic implements.

Mr. V. Ball, M.A., exhibited a number of objects of ethnological interest, collected in the districts of India, and also in the Nicobar and Andaman Islands. These included stone implements, battle-axes, an instrument like a boomerang used in killing small animals, but which was not capable of the return motion of the Australian boomerang, arrows, musical instruments, &c. There was a curious wooden figure, not an idol, but an effigy of some departed person, with a sort of girdle round the waist, which hung down like a tail behind; and the author thought it explained the ancient tradition of men with tails in those islands. Another singular item consisted of two ornamental skulls, in reference to which Mr. Ball mentioned that on the death of an Andamanese his body was placed on a tree, and as soon as his bones became bleached his skull was taken and ornamented, and first carried about by his widow, and afterwards by other members of his family. The collection included some photographs, one of which represented buildings on piles in water.

Mr. T. J. Hutchinson, lately her Majesty's Consul at Callao, read a paper entitled *Habits and Customs amongst some Tribes of*

Tropical Aborigines.—The tribes spoken of were some of those in West Africa and South America, with whom he had been acquainted during twenty-three years. The object of the paper was to show analogies in superstition and social barbarities, as well as in a sort of indigenous civilisation amongst people of different races dwelling on different sides of the globe, and with respect to whom there was no evidence of their having ever been in communication with each other.

M. Henri Martin, the French historian, read a paper written in French, entitled *Les Races anciennes de l'Irlande ; Utilité de l'Étude des Traditions qui les concernent pour l'Ethnographie de l'Europe primitive.*

The chairman, Lord Talbot de Malahide, said they must all be very much flattered at the interest which M. Henri Martin had taken in the ancient history and traditions of Ireland. He had shown in how many respects those traditions were connected with the traditions of the Continent, and how much light they threw on the migrations of the principal races which had colonised Europe.

Mr. H. H. Howorth made a communication on the *Spread of the Slavs.*—He said that if they excluded the Turks from Roumelia and Bulgaria, the Basques from Spain, and the various tribes of Finns from the north-east of Russia, they should have a tolerably homogeneous population left in Europe, which might be divided into the Celts, in the West ; the Teuton in the centre, and the Slavs in the East. The ethnology of the Slavs had only been recently treated on a scientific basis. They owed this to the researches of Count Polocki and Shafarick, the results of which, together with some of his own, he would bring before them. Patriotic Russians derived the name *Slav* from *Slova*—glory—a derivation improbable, when it was remembered that the name was very recent, not occurring before the sixth century. Shafarick believed that *Serb* was the oldest name of the Slavs, but that also was a modern name, and appeared to be of foreign origin. The oldest name of the Slavs was that by which they were known to the Germans—namely, *Wends*, a name occurring in classical writers under the form of Veneti. It was curious that the name *Slav* (slave) and *Servus* (Servian) should both indicate a people of servile condition. The Emperor Constantine, who gave the first account of the Servians, said that the name Servian was derived from the fact that they served the Roman emperor. About the Christian era, there was a great revival amongst the races of Eastern Europe. The power of various nomades of South Russia, who are now known to be of Aryan origin, was broken by the Romans. That enabled the Turkish and other nomades east of the Volga to invade Europe ; and they drove the Aryans to the centre of Europe, where they settled in the country about the Carpathians. This accounted for the fact that amongst the Poles and Bohemians the upper classes were entirely distinct from the lower, the Polish grandees being derived from the immigrants, while the peasants belonged to the old stock of Slavs. Afterwards the Turks and Huns pushed into Europe, driving the Slavs still further, and causing in the sixth century a great migration of them south of the Danube, where they overran Bulgaria, European Turkey, and the mainland of Greece. Others were driven to the Dalmatian coast and islands. The Emperor Heraclius, in order to protect himself, invited the Slavs of the Carpathians, called Croats, and the Serbs, to his assistance. The name Croat was derived from their living in the mountain districts. *Chrebet* and *Carparthian* being the same word. One section of the Serbs who lived on the river *Bosna* were called Bosnians ; another, who inhabited the Black Mountains, were called Montenegrins. A third section inhabited a district which was afterwards created into a dukedom by the Emperor of Austria, on whom they were dependent, and were called Herzegovinians from the German, *Herzog*, a duke. The rest of the Serbs formed the Servian community. All these were of one race, and had the same traditions, the only difference being that the Croats were converted to Roman Catholicism, while the Serbs became the converts of Greek priests ; and hence arose their existing religious differences. The Slavs of Eastern Thrace were conquered in the seventh century by a race of intruders closely allied to the Magyars of Hungary, and known as Bulgars. Their descendants were the dominant class in Bulgaria, and had the high cheek bones and oblique eyes of their race. The speaker concluded by describing the various migrations of the Russian Slavs.

Prof. Daniel Wilson, F.R.S.E., of Toronto University, read a paper *On some American Illustrations of the Evolution of New Varieties of Man.*—He said abundant traces were apparent of the intrusion into Europe in prehistoric times of one or more races superior alike in physical type and in the arts upon which progress depends to the primitive races. Furthermore it was assumed that an admixture took place between a fair, tall, intrusive race and the primitive Australioid savage of Western Europe, resulting in the so-called Melanochroi, still abundant in Britain, France, and elsewhere. On turning to the American Continent, they saw vast regions occupied exclusively, until a comparatively recent date, by tribes of savage hunters, upon whom the highly civilized races of Spain, France, and England have intruded with results in many respects so strikingly accordant with the supposed evolution of the Melanochroi of modern Europe, that they seemed to look upon an illustrative series of ethnological experiments carried on, on the amplest scale, and begetting synthesis altogether confirmatory of previous inductions. The intermingling of very diverse races at present taking place on the American Continent, was indeed complex and varied. Hybridity was the result on a great scale, and the process had already been perpetuated sufficiently long to beget important indications of the possible evolution of permanant hybrid varieties. A new race, as among the tribes of half-breeds of Manitoba, was seen as it were in the very process of evolution ; while, sheltered within the remote Arctic North, man could be studied among the Esquimaux in conditions closely analogous to those which are ascribed to post-pliocene if not to pre-glacial man. In the abrupt collision of the civilized races of Europe with the American aborigines, it had been taken for granted that the latter were doomed to inevitable extinction, and were to be replaced by the purely intrusive race of the Old World. A growing feeling was manifested now, alike in the United States and in Canada, in favour of the idea that the Indian population was not wholly disappearing by extinction, that a much larger amount of healthful intermixture and consequent absorption had taken place than unobservant critics had any conception of, and that the native Indian element was a factor in the population of the New World, destined to exercise an enduring influence on the ethnical character of the Euromerican race. If so, and the result was to be the perpetuation of permanent traces of the native American man in the mixed race of settlers by whom the vast forests and prairies of the New World were being converted to the uses of civilized communities, rivalling Europe in all the highest elements of progress, it would be no more than had been already recognised in the mixed races of Europe.

Mr. A. L. Lewis read a paper *On the Evils arising from the Use of Historical National Names and Scientific Terms.*—The propositions endeavoured to be established by Mr. Lewis were : (1) That there were at the first population of Europe certain primitive races, of which three are particularly described ; (2) that these races are so mixed at the present day that representatives of them appear not only in most European nations, but in the same families, and among children of the same parents ; (3), that notwithstanding this mixture, and the effects which it must permanently have, racial character displays an astonishing permanence ; (4) that this mixture, being so slow in its effects, and yet having become so general, has probably been at work, and for a very great length of time, so great that the peoples to whom the earliest history of Europe introduces us were probably nearly as much mixed as those of the present day ; (5) that it is desirable to discontinue the use of the political names of those people as ethnic names, and to employ others based on the physical characteristics of the individuals ; (6) that while physical characteristics are the only basis for a true division into races, yet in any practical application of this division, we must consider the influence upon individuals of different races of a community of language, whose history or tradition must not be lost sight of, although these things do not prove community of race, but only the contact at some time or other of the races to whom they are now common.

Prof. Huxley said the subject of the paper was one of importance, not merely on ethnological or scientific grounds, but because it was unfortunately the source of a great many practical fallacies which have had, and in fact still have, a very important political influence. He doubted very much whether there was any deliberate system of misnomer which was working more mischief in this world than the preposterous talk about the national qualities of the Celt and the Saxon. He had taken the liberty a number of years ago of getting himself into hot water by trying to awaken people's attention to what was the effect with regard to the use of these terms, and to the sort of mischief that was

being done by using them in the exceedingly inappropriate manner in which they were naturally used by political writers. His conclusions then were entirely in accordance with those which Mr. Lewis had just now brought before them. He (Prof. Huxley) believed that if there was a proposition in ethnology which was capable of historical proof it was that, so far as physical characteristics were concerned, the ancient Gauls—as was the opinion of the Roman and Greek historians—were persons of precisely the same physical peculiarities as the ancient Teutons known to the same historians. In fact, there was a most extraordinary correspondence to the phraseology in which the Teutons are described by a well-known writer, and those in which the earlier historians described the Gaulish invaders of the Roman Empire and the Greek Kingdom. That he believed to be beyond all question, and so far as physical characteristics went, he did not believe that there was a shred of evidence to show that the persons who spoke Celtic dialects at the time they made their appearance in Western Europe were in any physical respect different from those who spoke the older Teutonic dialect, and not only that there was no difference, but there was a most extraordinary resemblance, inasmuch as those stocks when they came into contact with the civilised world were described in the same terms—as sturdy, fair-haired people, with fair skins, and what he thought without any exaggeration may be described as a remarkable shortness of temper. He would not enter now into the interesting questions which Mr. Lewis had raised. The deliberate conclusions which were drawn from this subject with regard to the real distinction of race in our islands were, that the people of some particular race were marked by a tendency to certain social organizations and certain peculiar mental constitutions. Now he dared say that might be so. He could not—no person who was a professional zoologist could—fail to entertain the most exalted ideas of the influence of race, and he had no doubt there was great influence; but what he did very much doubt was whether they had the smallest means of knowing what at the same time was the amount of influence exerted on the people of this country by the different ethnological elements which compose it. Let any one who listened to the talk about national characteristics, and what was said about particular institutions being impossible for some of the people of these islands and possible for others—let him carry his mind back for the last twenty years and think what was at that time said about the German people. Great writers of public opinion at that time were never tired of enlarging on the saying of one great German, that while the Empire of France was on the land, and that of the British on the sea, the Germans had the empire of the air; but they proved themselves during the last fifteen years to be about as practical and hard-fisted a people as any that existed at the present time in the universe; and we did not hear anything of the Teutonic dreamers since the battle of Sedan. He believed that we knew so little about the races that it was impossible to disentangle what any particular nation was. We did on the other hand, know that there was a great deal of human nature in all kinds of men, and of social conditions which exercise an enormous influence. He thought he would endeavour to make out what in any given race at the present time was due to the pre-existing social and political relations—and when he had sifted that he would have some reason to talk of *residuum* as being the consequence of race influence. He himself did not believe, taking any one section of the British empire—whether Scotch or English or Irish—he did not believe that race has any appreciable influence upon their social and political condition of the present day. That was to say, his impression was that if the south-eastern parts of the British empire, the county of Kent, for instance, had been subject to just the same sort of conditions for 400 or 500 years as, he would say, Connemara and Galway, he should expect the results to be as nearly as possible the same; and it was a curious fact of ethnological study that those parts of Ireland which are supposed to exhibit in the most marked manner these characteristics, sometimes complimentary and sometimes uncomplimentary, were those in which it could be proved to demonstration that the Norman and English elements were most predominant.

Captain R. F. Burton read a paper entitled *Notes on the Tribes of Midian*.—The country once belonged to the Moabites, Ammonites, and Amalekites of Scripture, but the tribes now inhabiting it were comparatively modern. They were a mixed race. The inhabitants of the uplands were fairer in complexion and more fleshy and muscular than the dwellers in the lowlands, who were more dark and slender. Some of the higher classes were decidedly handsome, having erect muscular figures, straight features, black hair, and olive-coloured skins, fine eyes, restless and piercing, though their beards were rather thin. Longevity was rare amongst them in consequence of incessant fatigue, indifferent nourishment, and want of cleanliness. The inner man was not so easily described. Their chief characteristics were strong social affections, eternal suspiciousness, extreme pugnacity, and proportionate revengefulness. Their sociability was extreme, and they made great sacrifices for one another.

Prof. W. H. Flower read a paper *On the Methods and Results of Measuring the Capacity of Crania*.—Of all the measurements by which they could determine the difference between the human skulls of people of one race and of a foreign race, perhaps the most important was that which gave the cubic capacity of the great cavity of the skull which contained the brain. Many ways of ascertaining it had been tried. Some persons laid great stress on the weight of the brain, but for his part he thought that on the whole if the capacity of the skull could be got it would be more valuable. The weight of the brain differed very much according to the age or physical conditions of the person when he died, and there were certain diseases which went to increase the specific gravity. But when the actual capacity of skull was found they had the actual capacity of the brain at the time of health. There was another very important reason why they laid stress on obtaining the capacity of the crania in preference to the other method. It was because all their museums now contained a number of skulls from different parts of the earth, some of which were very inaccessible to scientific observation, and it was, of course, impossible to ascertain the actual weight of the brains of these people after death. Then, again, how could they get the capacity of the skull by the weight of the brains in cases where the races had become extinct, such as the Tasmanians, many of the Polynesians, the ancient Britons, and the ancient Irish, and others, specimens of whose skulls they possessed, and by which they could ascertain the capacity of the brain? He supposed he would be expected to say at once whether he attributed any great and direct importance to the weight and age of the brain as an indication of intelligence. Well, he thought it was one of the very many points that had to be considered in this question; but he thought there were a great many other things to be remembered in this view of the question. For instance many people had large brains and did not know how to use them, and some who know how to use them did not try to do it. They would see that many of the races that were naturally considered the higher races, and had taken the lead in the civilisation of the world, had undoubtedly larger cranial capacities than the peoples who were at the bottom of the ladder of civilisation. He would never accept the mere fact of a man's head being large as an indication of superior intelligence, but it was one point to be considered. The measurement of the skull was not only an important but it was also a difficult work, more difficult in fact than a great many people supposed, and a great many of the uncertain results that had been obtained on this subject were owing to the persons who had taken the matter in hand not having yet discovered the best and most certain method of carrying out the investigation. A large number of measurements published were only of an approximate value, owing to the numerous fallacies and difficulties experienced in arriving at a satisfactory method of measurement. Nothing, apparently, could be easier than to take a skull and stop the cavities, and pour some fluid into it and then pour it out and measure it, but they could not do this with the skull, as the bone was very porous and full of minute invisible holes, through which the fluid soaked as it would through a sponge. It was only by making the skull waterproof that they could seek to measure its cavity by a fluid. He had a skull by him which had been so prepared. The large holes had been filled with wax and the skull soaked in melted paraffin, which filled up the minute cavities, and when it was cooled it was as impervious to any fluid as delf. But the materials that had to be used in testing the capacity of the skull must be something solid. Various things, such as shot, grain, &c., had been used. He would pass over the various methods that had been tried and failed, and which would be found recorded in the *Transactions* of the Anthropological Society of Paris, and speak of two methods which at the present time meet with the greatest amount of success. One was the method of Mons. Broca, and the other the method of Mr. Busk. The latter had shown such good reasons for his plan that he thought it particularly safe to try it, and after doing so he had adopted it with some modifications.

He filled the skull with mustard seed well shaken, and pressed into it with the thumb, and then poured into a long wooden box with glass sides in it, in which it was well shaken and pressed down. The figures on the glass indicated the spaces filled. This he thought was the most satisfactory way as yet invented, and they could hardly hope for better. He always kept his experimental skull by him when measuring other skulls, in order that he might occasionally go back on it to see if he had gone wrong. Now, as to the measurement of the skulls of the different races of the human family, a very important point to consider, and a very difficult one, was the sexes, because there was a great difference in the size of the skulls; a much greater difference than there was between men of different races. To get the average of any race they must get a large number of skulls, and he must say their collection was very insufficient at present. According to a comparison between the skulls of sixty-three men of various races, and skulls of twenty-four women, the size of the woman's skull to the man's was as 854 to 1,000. The largest normal skull he had ever measured was as much as 2,075. He knew nothing of its history. It might have been the head of a great philosopher, but unfortunately they were not in the habit of getting the heads of philosophers in their museum. Nearly all the English skulls were those of persons in the lowest ranks of life. It was these they had to compare with the specimens of other races. The smallest head he had measured was 960 centimetres, and that belonged to one of those peculiar people in the centre of Ceylon, who were now nearly extinct. The largest average capacity of any human head he had measured was that of a race of long, flat-headed people on the West Coast of Africa. The Laplanders and Esquimaux, who were a very small people, had very large skulls. The latter gave an average measurement of 1,546. He then came to the English skull, which was nearly the same size—1,542; but, as he had said, they belonged to the lower grades of English skulls. He could not tell them anything about Irish skulls, for there was not a single specimen of the Irish skull in any London museum. The inhabitants of the Canary Islands give a capacity of 1,498; the Japanese, 1,486; the Chinese, 1,424; the modern Italian, 1,475; the ancient Egyptian, 1,464; the true Polynesians, 1,454; negroes of various kinds, 1,377; the Kaffirs, 1,348; Hindoos, 1,306. They then came to the Australian aborigines, who were amongst the smallest, only giving an average of 1,283. There were two races still below the Australians, namely, the Andamanese, who were a very diminutive people, with a capacity of 1,220, and the Veddahs, of Ceylon, who had an average skull.

The President (Prof. Huxley) said he might, without hesitation, offer the best thanks of the Section to Prof. Flower for the important and interesting paper he had just read. Persons not ordinarily occupied with scientific pursuits might not be aware of the amount of care that had to be taken when it was desired to do any good in scientific matters in obtaining data, which data would, when obtained, pack into the very smallest possible results. It would be seen what care was required to obtain measurements of the cubical contents of the skulls, and yet the whole of the labour, if Mr. Flower published his paper, as he hoped he would, would go into the space occupied by the three or four rows of figures. There was one very interesting question he wished to put to Mr. Flower—whether it was possible to establish not only a series of absolute measurements of the capacities of the skull, but also some kind of index of capacity in which can be expressed the ratio of capacity of the skull to the stature of the person to whom it belonged, or if it was impossible to obtain that, yet even to obtain such data as would show the relation between the contents of the skull and the length of the part of the skull which was, as it were, the foundation of the skull.

Department of Anatomy and Physiology.

On a Direct Method for Determining the Calorific Power of Alimentary Substances, by J. A. Wanklyn and W. J. Cooper.—The amount of oxygen consumed by an organic substance being the measure of its heat-producing force, the importance to physiologists of a direct and rapid method for measuring the consumption of oxygen in organic fluids is obvious. It is well known that an elementary combustion will effect this object, but, as is likewise well known, the great difficulties which beset it render it unavailable for physiological researches. The process by which we obtain all the results of an elementary combustion can be completed in about an hour, and in the course of our experiments on various organic substances we have been enabled to get an amount of oxygen absorbed which is equal to the theoretical quantity required by the substance operated upon. In point of fact, we have so modified Forchhammer's process as to make it work in a satisfactory manner.

Forchhammer's process, as hitherto practised, does not effect anything approximating to complete oxidation down to carbonic acid and water, as was illustrated by some experiments published by Frankland and Armstrong in 1868 (*vide* Chem. Soc. *Journ.*, vol. vi. p. 82), which we quote:—

Name of substance (30 parts dissolved in 1,000,000 parts of water).	Oxygen absorbed during six hours.	Oxygen required for complete oxidation.
Gum arabic	0·35	35·5
Cane-sugar	0·15	33·7
Starch	0·30	33·5

showing that, as usually carried out, the oxidising process does not avail to accomplish more than about one-hundredth part of the task set before it.

The modifications whereby we have completely altered the character of the Forchhammer process are as follows:—

Instead of simply mixing the standard solution of permanganate with the water to be examined, we *distil* a given volume of the water (say 1 litre) with a considerable excess of standard solution of permanganate, and thereby get more oxidising action than in the ordinary operation. We find advantage in having the liquid strongly alkaline during the distillation; but we render it acid before titrating the residue.

Mr. Lawson Tait, F.R.C.S., read a paper entitled *Note on the Occurrence of a Sacral Dimple and its Possible Significance.*—Some years ago he noticed casually the occurrence of a curious pit-like depression or dimple in the skin over the lower bones of the sacrum amongst the patients of the hospital for women to which he was attached. He paid no special attention to it until about two years ago, when an instance came under his notice of a woman in whom it was well marked, and in all of whose children it was to be observed. Three of them, all girls, had it strongly pronounced; and in one, the eldest, about eight years of age, he found the best marked instance he had yet seen. It was quite a centimetre in depth, and it expanded outward, so that its mouth had a diameter of about 13 millimetres. This circumstance induced him to make observations as to the frequency of its occurrence in some hundreds of women passing under his notice, and he found that in 55 per cent. no trace of it was visible, that in 22 per cent. it was faintly marked, and in 23 per cent. it was well marked. Occasionally two depressions were present instead of one, both being in the middle line always, and situated from half an inch to an inch and a half apart. The average age of the women, in whom it was well marked, was slightly over 32 years, whereas that of those in whom it could not be observed was nearly 45, from which he concluded that it had a tendency to disappear with advancing age. The same inference was to be drawn from observations made by Dr. A. H. Carter at the Children's Hospital. The prevalence of this curious mark was suggestive that it must be a remainder from some embryonic process connected with the neural canal and its closure. The explanation which he was about to offer of its significance must be viewed charitably, and as the result of an accidental observation. Mr. Tait then gave an account of the appearance presented by the body of a kitten remarkable as having no tail. The mother was a favourite cat in a district where there were no tailless tom cats. The lady who presented it to him told him that during the whole of the kitten's brief life water was seen to run from the spot where the tail ought to have been, which water was industriously removed by the mother. The kitten took nourishment, and moved about after the fashion of new-born kittens, but did not appear to use its hind legs. On examining the body he found a small aperture through which fluid could still be pressed, and on opening this carefully he found that it passed through a deficiency of the neural arches of the sacrum, directly into the cavity of the spinal cord. At this point the cord broke up into its ultimate branches, and the tail was represented just as it is in other tailless vertebrates, the neural arches being inclosed, and the caudal vertebra being represented by only three elongated centra, enveloped in a slight fold of skin, just as in the Manx cat and in the Guinea pig. The study of a number of human sacra revealed the

curious fact that they differed very materially. The sacrum is generally figured as being composed of five vertebral elements, but he had frequently found six, and in one case seven. A friend had told him that he had seen one with only four. Of the five usual sacral vertebræ the two lower are generally figured as having gaps in the neural arches, but it was his experience that it was more usual to find the gap also in the third, and he had seen sacra in which there was only one neural arch. He had not studied the sacra and coccyges of any of the tailless vertebrata but the guinea pig, but even in it he had found curious variations, chiefly in the length and number of the coccygeal centre, and he assumed that in such an early type such variations must of necessity be less marked than in the later ones. He thought it likely that spina bifida was not confined to the human animal. In the human animal its most common seat is the sacral and lower lumbar vertebræ. It was fair to assume, therefore, that in a tailed vertebrate it would occur more frequently in the tail than elsewhere. If this be the case, and if, likewise, it ever does occur amongst animals not domesticated, it will most certainly occur occasionally that a tailless vertebrate will, by reason of a spina bifida, be born of properly tailed parents. If the deformity was sufficiently high up to destroy the tail, yet not high enough to interfere with the nervous supply to the posterior limbs, then he might survive in the struggle for existence, provided the tail was not essential to life. They could easily imagine that under favourable circumstances a monkey born with this change in his economy might strike out a new line of life, breed tailless children, and by the change of other structure necessitated by the new method of life, introduce a new variety of monkey. Those who were still opposed to the evolutionary view of creation might sneer at this as a series of "ifs," but they must be reminded that the whole of life—individual and collective—is but a change of circumstances, precedent to any one of which an "if" can be introduced, and that had "if" really been effective the whole chain would have been different from that point. What, then, was the significance which might be attached to the sacral dimple which was the subject of the paper? Looking at it surgically there could be no question that it was a cicatrix. What he ventured to suggest was that it was the hereditary cicatrix of the *spina bifida* by which the human tail had been lost. Such a suggestion was of course open to ridicule, but only, he ventured to think, in the minds of the incautious or the ignorant. That man was once a tailed vertebrate was beyond doubt, and that he lost his tail was, of course, equally evident. He was not the only vertebrate who had lost a tail, and in whatever way those tails had been lost they had evidently all been lost in the same way. Now, curiously enough, they had an animal living in one of the sister isles of which not a genus, nor a species, nor a variety, but a mere family had lost its tail; he referred of course to the Manx cat. He had obtained some of these cats from the Isle of Man, and he was quite certain that if they knew the history of this curious family of cats they should know exactly how all the vertebrates which are now tailless became so, and he felt very confident that the Manx cats lost their tails through the occurrence, within very recent times, of a tom cat with a *spina bifida* placed exactly where it was in the case of the kitten he had referred to. A limited area like Man would present the most favourable circumstances for the protection and propagation of such a variation; for he (Mr. Tait) had already elsewhere pointed out that the bushy tail of such animals as the cat served chiefly for the purpose of maintaining temperature, and in the mild and equal climate of the charming island a tailless cat would have little hardship to endure. It could not have survived, however, in any place where snow lay long on the ground. In the few Manx cats he had examined he had seen no trace of dimple, nor had he seen any appearance of it in the guinea pig; but these negative results did not seem to him to be important. What would be important would be the examination of a number of tailless monkeys, especially young ones. If in these no dimple was to be found, then he feared they should have to look forward to some other history for this curious cicatrix.

The president said he was not inclined to take the same view as Mr. Lawson Tait as to the means by which the tail in the human species had been got rid of. It appeared to him that what Mr. Tait had described was more likely to be the remains of infantile *spina bifida*: It was all very well for human beings to object to their having tails, but, as a matter of fact, they had tails, though they had disappeared to very small dimensions, in which they now existed in the human family.

Department of Zoology and Botany.

On the Remains of a Permian Fauna in North America, by Prof. E. D. Cope, U.S.A.—Prof. Cope described the remains of a fauna, now extinct, which had inhabited North America during the period of geologic time next succeeding the coal measures, which is known as the Permian. He had first ascertained its existence through specimens sent from Illinois in 1875, but had discovered much larger deposits of similar animals in Texas. The characters of the latter showed that they were to be referred to the classes *Reptilia* and *Batrachia*. A number of generic types of reptiles were mentioned, most of which are characterised by the notochordal vertebræ. He particularised the characters of the dentition of *Diadectes* and *Bolosaurus*, where the teeth are transverse to the long axis of the jaws. He entered more fully into the structure of *Clepsydrops*, Cope, where almost the entire skeleton had been discovered. This includes clawed lizards, with large canine teeth and several incisors, humerus without condyles, but with supracondylar foramen; reptilian posterior limb; boat-shaped pelvis, without obturator foramen, and with the neural spines of the sacral vertebræ greatly elevated (in *C. natalis*). There are small intercentra between the inferior parts of the adjacent centra, which support the chevron bones in the tail.

The Batrachia display remarkable characters of the vertebral column. In *Cricotus* the intercentra are developed so as to resemble centra, so that the column appears to consist of two kinds of vertebræ alternating with each other. A neural arch of the caudal series stands equally on centrum and intercentrum, but the intercentrum only bears the chevron bone.

Sir J. Lubbock read a paper *On the Habits of Ants*.—The author observed that he had kept about thirty species of ants in confinement. They throve well, and he had some specimens which he had kept since 1874. They were probably bred in the previous year, and would now, therefore, be five years old. He also referred briefly to the other insects which were kept by ants in their nests, and especially to aphides, some species of which are kept and carefully tended by the ants throughout the winter, though at that season they are not of any use. He referred shortly to his experiments on the senses of ants. Their sense of smell is very delicate, though much more so in some species than in others. On the contrary, he had never observed any proof that they are capable of hearing. As regards sight, he had been able to satisfy himself that they were capable of distinguishing colours, and that they are, for instance, very sensitive to violet. The ants of a nest not only knew one another, but they remembered one another even after a year's separation; and he recorded some experiments by which he attempted to ascertain how the recognition is effected. He also referred briefly to the insects which are domesticated by ants, and gave a short account of the slave-making species, which are (at least in one case) entirely dependent on their slaves, and would perish even in the midst of plenty if left to themselves. He kept some of these ants, however, alive for months by giving them a slave for an hour a day to clean and feed them. The communities of ants, he said, offer numerous analogies to those of men, and the difference in the habits of the various species of ants are also in this respect not without interest. The slave-making ants, indeed, represent an abnormal and, perhaps, only a temporary state of things, for it is not impossible that the slave-making species will eventually find it impossible to compete with those which are more self-dependent and have reached a higher phase of civilisation. But putting these species on one side, we find in the different species of ants different conditions of life curiously answering to the earlier stages of human progress—namely, the hunting and pastoral, and even to the agricultural. For instance, some species, such as *Formica fusca*, live principally on the produce of the chase, for, though they feed partly on the honey-dew of aphides, they have not domesticated these insects. These species probably retain the habits once common to all ants. They resemble moreover the lower races of men who subsist mainly by hunting. Like these, they live in comparatively small communities, and the instincts of collective action are little developed among them. They hunt singly, and their battles are single combats like those of early history. Such species as *Lasius flavus* represent a distinctly higher type of social life. They may literally be said to have domesticated certain species of aphides, and may be compared to the pastoral stage of human progress, to the races which live on the produce of their flocks and herds. Their communities are more numerous, they act more in concert, their battles are no mere single combats, but

they know how to act in combination. Sir John was disposed to hazard the conjecture that they will gradually exterminate the mere hunting species, just as savages disappear before more advanced races. Lastly, the agricultural nations may be compared with the harvesting ants, none of which, however, live in our country. When he first began keeping ants, Sir John surrounded the nests by moats of water. This acted well, but the water required continual renewing, especially of course in summer, just when the ants were most active. At length, however, in considering the habits of ants and their relation to flowers, another plan occurred to him. The hairs by which plants are clothed are of various forms, and fulfil various functions. One is to prevent ants and other creeping insects from climbing up the plants so as to obtain access to the flowers, and thus rob them of their honey. It occurred to him, therefore, that instead of water he might use fur, arranged so that the hairs pointed downwards. This he found to answer perfectly; and he mentioned it specially because the same arrangement may perhaps be found practically useful in hot climates. It is, of course, very possible, indeed, that the tropical species might be able to climb up the fur, or that for other reasons the plan might fail—the fur itself, for instance, might be devoured—but, at any rate, the experiment would be worth trying. It is generally stated that the queens alone lay eggs. This, however, appears not to be the case. The workers do sometimes, though only as exceptions, lay eggs; but it is curious that their eggs always appear to produce males.

Sir Walter Elliott made a few observations *On the Annual Increase of the Common Vole (Arvicola agrestis) of late Years*.—In the spring of 1876 they appeared in such numbers in the hill pasture farms of the border districts between England and Scotland, and parts of Yorkshire and Wensleydale, as to destroy the grazing ground on which the sheep depended in spring, causing serious loss to the farmers by impoverishment and death of stock.

Dr. R. H. Traquair read a paper *On the Genus Ctenodus* (Agassiz).—The object of the paper was to suggest the great probability of the identity of the genus Campylopleuron of Huxley with Ctenodus of Agassiz. Should that be the case, Ctenodus will differ more from the old red sandstone genus Dipterus than is usually supposed, as in Camplyopleuron there is, as in the recent Ceratodus, a continuous dorsocaudal fin.

Aberrant Sacrum connected with the Oblique Pelvis.—Dr. Allen Thomson, F.R.S., showed a number of sacra exemplifying the irregularity which he regarded as the cause of oblique pelvis. He referred to similar developmental irregularities occurring in the lower animals, and exhibited one specimen—namely, the skeleton of a wombat—where the irregularity was one-sided, as is the common cause of the oblique pelvis in the human subject.

Prof. Macalister, Dr. Lawson Tait, and Dr. Harvey took part in the discussion, and all the speakers agreed that the developmental cause must be considered the usual one, and in ignoring the occasional occurrence of this form as resulting from disease.

Mr. R. W. Sinclair read a paper *On Recent Additions to the Irish Lepidoptera*.—In his paper he mentioned fifty-four species new to the Irish list. Mr. Sinclair said that it was very remarkable that of the large number of fen and marsh insects that occur in England, hardly one-third occur in Ireland; for instance, in the genus Leucanidæ, out of the thirty-one English species only thirteen are Irish.

Mr. C. Spence-Bate, F.R.S., presented a report *On the Present State of our Knowledge of the Crustacea*.—This paper formed Part 4 of the series, and was on development. Mr. Spence-Bate also read a paper *On the Willemœsia Group of Crustacea in the "Challenger" Collection*. These had all been stated to be animals without even the rudiments of eyes, and appeared to correspond very closely with the genus described from the Mediterranean as Polycheles. The author pointed out that not only were the animals of the group not blind, but that they had eyes of varying proportions. The species of these genera were taken at thirteen different stations, at depths varying from 120 to 1,900 fathoms, and chiefly on a soft bottom of mud or globigerina ooze, and he thought they might safely infer that the entire group are dependent upon the nature of the bottom for their existence, and that their general form is in accord with the habits of an animal that burrows in the deep sea ooze, which has been selected as its best adapted food.

Mr. H. H. Howorth, F.S.A., read a paper *On the Extinction of the Mammoth in Siberia*, the principal object of which was to deal with the difficulties that surrounded the explanation as to the mode in which the animal became extinct. After looking at the problem from every side, he had come to the conclusion that there had been a sudden and violent change of climate in Siberia which had frozen the previously soft ground, and had also preserved the mammoths as in a huge meat safe. Although the mammoth had even originally lived in the place where he was now found, it was impossible that he could live there now, owing to the absence in that part of the food which would be necessary to sustain him. Such trees as he used to live on were only now to be found about 500 miles from the spot where his remains were discovered. The natural corollary that followed from this theory was that something similar must be postulated with regard to other regions. The conditions in which the elephant was found in Siberia were precisely similar to those in which it was found in the north-western part of Russian-America, and precisely the same as those in the Great Lakes, where the mammoth itself was found, and it could not, therefore, be doubted that the mammoth lived in Europe and America with the same food and surroundings as it did in Siberia.

SECTION E.—Geography.

Captain Burton read a paper *On the Land of Midian*, giving an account of his recent explorations in that region, which have already been referred to in these pages.

Dr. Phené read a paper *On the acquisition of Cyprus, and Observations on some Islands in the Levant with Reference to Recent Discoveries*. The author, who had recently made a prolonged and careful voyage of research in the Levant, described the physical features of the islands of Chios, Mitylene, Lemnos, Imbros, Thasos, and Samothrace. He selected Samothrace for ascent, and was, so far as he could learn on the island, the only European, not being a native, who had made the ascent, which was very difficult. The height was slightly over 5,000 feet. The climates, culture, and salubrity of the different islands were dwelt on. Cyprus had a variety of climate, so that the debility produced by the heats in the south could be relieved by a retreat to the northern coast, which was cooled by the breezes coming from the Karamanian Mountains, while in some inland parts were rich woods abounding with game and objects for the chase.

Major Wilson, Director of the Ordnance Survey of Ireland, also read a paper *On Cyprus*.

Lieut. Kitchener, R.E., in a paper *On a Survey of Galilee*, gave an account of the progress of the work of the Palestine Exploration Fund in that region. The great work on which the Society has been employed for the last six years, is a map of Palestine on the model of the Ordnance Survey of England and Ireland. The map of Palestine on the one-inch scale has now been completed. Lieut. Kitchener then detailed the progress of himself and his predecessors in the work of surveying and in exploring ancient remains and sites. At the end of this year, if funds are available, an expedition will start to explore the sites of the most sacred scenes of the New Testament history—the northern shores of the Sea of Galilee, where undoubtedly Capernaum, Chorazin, and Bethsaida still exist. In addition to this the expedition will make a thorough survey of the unknown country forming the eastern shores of that sea, on the same scale and with the same accuracy as the present survey. They hope also to rescue from the hands of that ruthless destroyer, the uneducated Arab, one of the most interesting ruins in Palestine, the Synagogue of Capernaum, which is rapidly disappearing, owing to the stones being burnt for lime.

Mr. W. H. Dall, of the U.S. Survey, gave an interesting account of its recent exploration in Alaska, some of the results of which we have referred to in NATURE.

SECTION F.—Economical Science and Statistics.

A paper by Prof. Jevons was read *On the Periodicity of Crises and its Physical Explanation*.—Various reasons, such as wars, trades unions, luxurious living, &c., had been given as explanations of the now constantly recurring depression of trade. Such explanations he (Prof. Jevons) did not consider satisfactory. Depression of trade had occurred during the present century with remarkable regularity at intervals of ten years. Sir John Herschel had attempted to find a connection between certain meteorological phenomena and the price of corn in Europe. If we traced backward from 1866, when a very great depression of

trade took place, it was to be observed that in 1857 an equally severe commercial crisis took place both in England and the United States. The year 1847 was memorable for the excessive number of bankruptcies, and in 1839 and 1836 crises took place in England, while in 1837 the crisis took place in the United States. Some exceptional cause appeared to have broken up the crisis into minor crises. From 1837 they progressed 11 or 12 years to the great bubble year, 1825. The paper went on to show that during the last 165 years there had been 16 great commercial crises at intervals of about ten years and concluded by stating that the fact of periodicity of commercial crises was so strong that it could not be doubted, and the question of a physical cause was only a matter of speculation.

SECTION G.—Mechanical Science.

Mr. G. J. Symons, F.R.S., read a paper *On the Rainfall of Ireland*, in which he mentioned as a remarkable fact that the Irish hills do not exhaust rain clouds as the English hills do. With the exception of a dry central area around Dublin, the rainfall all over Ireland may be taken to be the same. At present, instead of the greatest rainfall being in the south-west, or in Galway, we have the wettest spot of all (with one exception) in the south of the county Down, the very place which, theoretically, might be expected to be almost the driest part of Ireland. That shows that it is really a question more of the elevation of hills than of geographical position. He exhibited a map showing the number of rain stations established for the observation of the rainfall. He had succeeded since the meeting of the Association in Belfast in obtaining the services of a large number of gentlemen volunteers throughout Ireland who had taken charge of the rain-gauges supplied to them, and had engaged to register their observations. There were still large districts, however, in which he had not been able to establish rain-gauges, and the observations were, therefore, necessarily defective as to the average rainfall. There was a large district in the neighbourhood of Longford without a single station. The same could be said of other stations, where it was essential that observations should be taken. If he could induce some gentlemen having property in these neighbourhoods to take charge of rain-gauges, Ireland, instead of having to depend upon ten stations, as it did not many years ago, would be fairly represented both geographically and physically.

Mr. W. H. Preece, C.E., read a paper *On Recent Advances in Telegraphy*, with the effect of showing that improvement in telegraphy was never more active in England than it has been since the Government managed the business. Having indicated the improvements effected, the paper concluded:—"The control of Parliament and of the press exercises a far more disciplinary and supervising power on the management of a Government department than any half-yearly meeting of shareholders, or occasional committee of investigation."

Mr. Wigham read a paper *On the Irish Siren Fog Signal*.—The Irish siren is adapted either for steam or compressed air, and differs from those made in America and in England in being driven by a species of small turbine actuated by the current of the steam or air by which the instrument is sounded, the rate of rotation being controlled and rendered uniform by a simple governor, a much less complex arrangement than the somewhat cumbrous mechanism which has heretofore been used. The Irish siren is applicable to steamships as well as to lighthouses.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Prof. Helmholtz is succeeded in the rectorship of the Berlin University by Prof. Zeller.

The University of Halle is receiving valuable additions in the form of new edifices for the various departments. The necessity of new buildings with modern appliances has long been felt, and the want is being met at present by the erection of a library, two new clinics, and pathological, physiological, and anatomical institutes.

Sweden possesses at present one of the most thorough systems of education in Europe. It is difficult to find a district where one per cent. of the population are unable to read and write. The common schools of the country cost about 220,000*l*. yearly. There are numerous institutions for agricultural and technical education, and two universities at Lund and Upsala, which compare fairly with the German universities, in regard to the amount of valuable research carried out in various departments.

Spain has at present ten universities. Those of Madrid, Barcelona, and Granada possess each five faculties—law, medicine, pharmacy, the exact sciences, and philosophy and literature. In those of Salamanca, Seville, and Valencia pharmacy is absent. Saragossa has law, medicine, and philosophy; Santiago and Valladolid law and medicine; and Oviedo law only. The professors number 414, and the students 15,000; Madrid alone contains, however, 76 professors and 6,500 students. Technical education is provided for by schools of mines at Madrid and Almaden, the agricultural schools of Madrid and Cordova, the veterinary institutes of Madrid, Cordova, Leon, and Saragossa, the School of Architecture, and the school of Civil Engineers at Madrid, as well as polytechnics in various cities. Preparatory schools number 63, with 30,000 students, and elementary schools number 28,000, with an attendance of 1,400,000.

SCIENTIFIC SERIALS

American Journal of Mathematics, vol. i. No. 2.—On the application of the new atomic theory to the graphical representation of the invariants and covariants of binary quantics, by Prof. Sylvester.—Researches in the lunar theory, II., by G. W. Hill.—Bipunctual co-ordinates, by F. Franklin.—Desiderata and suggestions. No. 2. The theory of groups; graphical representation, by Prof. Cayley.—On the electric potential of crystals, by W. E. Story.—Théorie des fonctions numériques simplement périodiques, by Prof. Ed. Lucas.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, August 19.—M. Daubrée, president.—Meridian observations of the minor planets made at the Paris Observatory during the second quarter of 1878; communicated by M. Mouchez.—Experimental researches on the nervous sudoral fibres of the cat, by M. A. Vulpian.—On a new species of mineral named thaumasite, by Prof. Nordenskjöld.—On the alterations caused by the phylloxera in the roots of the vine, by M. A. Millardet.—Elements of the planet 148, Gallia, by M. Bossert.—Second note on the employment of identities in the solution of numerical equations, by M. Desboves.—Spectrometric study of some sources of light, by M. Crova.—Ambulant electric sparks, by M. G. Planté.—On a telephone able to transmit sounds to a distance, by M. Righi.—On a new improvement in the peroxide of manganese and sal ammoniac pile, by M. Leclanché.—On the dissociation of metallic sulphurs, by MM. Ph. de Clermont and J. Frommel.—On the value of magnesia as an antidote to arsenious acid, by the same.—On two beds of phosphatic lime in the Vosges, by M. P. Guyot.—Researches on the nutrition of insects, by M. L. Joulin.—Researches on the relations of weight which exist between the bones of a skeleton of the goat, by M. S. de Luca.—The new meteoric mineral, Daubréelite; its constitution; its frequency in meteoric irons, by Mr. Lawrence Smith.—New molluscs of the Parisian tertiaries, by M. Stan Meunier.

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